THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XXXII.—*On the Crumpling of the Earth's Crust;** by WILLIAM B. TAYLOR.

THE causes and conditions of the vast series of movements which have taken place throughout the earth's crust, present perhaps the most fundamental problem of physical or dynamical geology. And yet this problem is one of the most difficult and unsettled in the range of geological inquiry. It is indeed a very complex one, embracing several quite distinct features. The enormous lateral compressions which have everywhere crushed, folded, and contorted the stratified rocks, culminating in vast uptilted and fractured mountain ranges, may or may not have been instrumental in the elevation of continents, and the depression of ocean-beds. The alternate subsidence and emergence through miles of vertical oscillation of continental areas—successively repeated (by the agency of internal or external influences)—may or may not be correlated with equivalent upheavals and submergence of the present oceanic areas. And whether we infer with some geologists that ocean and continent have more than once changed places, or on the other hand conclude with Dana, and others, that the skeletons of the existing continents were rudely blocked out and the foundations of the great deep laid down in pre-archæan times, we are on either supposition beset with grave and peculiar alternative perplexities. The hypothesis of Joseph Le-

* Read before the Philosophical Society of Washington, May 23, 1885.

Conte, and others, that the tangential stresses of secular lateral compression have resulted in very great differences of thickening in the crust, deepening simultaneously the foundations of the mountains as they were slowly protruded upward, while leaving the great depressions proportionately attenuated, so that the internal topography of the shell is an obverse corresponding closely with its external contours,—although an hypothesis framed for the avoidance of supposed hydrostatical difficulties in the conception of a layer of approximately uniform thickness, floating on the liquid abyss,—is yet on both geological and physical grounds exposed to at least as weighty objections.

It is proposed in this communication to consider only the probable origin of that great contraction of the terrestrial crust so widely manifested in its plications or crumplings, increasingly marked as we approach the great oceans.

The liquidity of our globe, and the relative thinness of its incrusted envelope,—as attested by all legitimate *geological* induction,—will be assumed without misgiving or hesitancy; and the supposed mathematical arguments for its solidity—ignored as essentially fallacious and wholly inconclusive. It must be lamented that the professional mathematicians have as yet contributed nothing to the advancement or to the extension of geologic theory:—not from lack of analytic skill in a Thomson, or a Darwin, but from the want of proper data to justify the conclusions they have so boldly hazarded.

On the universal recognition from numerous evidences that our earth is a cooling globe, still highly heated within, the suggestion by Elie de Beaumont, in 1830, that "the inequality of cooling [between the interior and exterior portions] would place the crusts under the necessity of continually diminishing their capacities . . . in order that they should not cease to embrace their internal masses exactly," and that this condition "may with great probability completely account for the ridges and protuberances which have been formed on the external crust of the earth:"*—this seemed so natural and obvious an explanation, that it commanded a very general assent among geologists.†

* Phil. Mag., Oct., 1831, vol. x, pp. 263, 264.

† "Elie de Beaumont and some other geologists have attributed these effects and especially the elevation of mountains to the contraction of a cooling globe, and this appears to be the only one adequate for the results. . . . In attributing the plications of the earth's crust and the elevation of most mountains to a lateral pushing movement within the crust, there is nothing hypothetical. The statement is the expression simply of a fact. The conclusion that this tension [compression] is due to the contraction of a cooling globe has not yet been fully established. It is here adopted, because no other that is at all adequate has been presented."—*Prof. J. D. Dana, Manual of Geology*, 1863, part iv, chap. vi, sect. 1, pp. 721 and 725.

There were not wanting some however who questioned the sufficiency of the cause to accomplish the observed results; and our colleague Captain C. E. Dutton some ten or twelve years ago with great acuteness pointed out the entire inadequacy of the "Contractional Hypothesis," from an engineering stand-point.*

Mr. Robert Mallett, a zealous supporter of the contractional hypothesis, estimated that the diameter of the earth is now about 189 miles less than it was when entirely fluid.† That is, that the surface has collapsed ninety-four miles. Liberal as is this allowance (a surface contraction linearly of $\frac{1}{42}$), it would still be utterly insufficient to represent the actual average of compression. But in point of fact, the whole amount of cool. ing, and therefore of possible contraction by cooling, has been very much less than has generally been assumed.‡

1. No permanent crust could have been formed upon the molten globe until convective currents had well nigh ceased; that is, until the entire mass had cooled down very nearly (say —within a hundred degrees F.) to the congealing point.§

2. After the formation of a permanent crust, the greater part of the heat escaping through it would probably have been the "latent heat" from consolidation of new accessions to its inner surface, with little reduction of the sensible temperature of the inner mass.

3. Whatever *expansion* by freezing or consolidation of such accessions to the inner surface might occur, would to their extent counteract the effects of interior shrinkage.

4. The further escape of the encased heat through several miles of so poor a conductor as the earth's crust, must have since gone on with extreme and ever lagging slowness; and even through the lapse of a million centuries, must have been

* This Journal, 1874, vol. viii, pp. 113–123. The Rev. Osmond Fisher, in his Physics of the Earth's Crust (8vo, Lond., 1881), has also successfully attacked the hypothesis, chapter vii, pp. 73–75.

† Phil. Trans. Roy. Soc., June 20, 1872, vol. clxiii, pp. 147–227.

‡ "If we were to assign thirty miles as the diminution of the earth's mean radius since the first formation of a cooled exterior, we should probably reach the utmost limit consistent with Fourier's theorem."—*C. E. Dutton*, this Journal, 1874, vol. viii, p. 121.

§ Mr. William Hopkins, the author of the unfortunate "precession" argument against a fluid earth (or rather against a flexible earth-crust), in the same memoir, well remarks: "If the matter composing the globe was originally in a high state of fluidity from heat, the process of cooling would undoubtedly in the first instance be by circulation." And even supposing solidification to commence at the center, he maintains: "The superficial parts of the mass must in all cases cool the most rapidly; and now (in consequence of the imperfect fluidity) being no longer able to descend, a crust will be formed on the surface; from which the process of solidification will proceed far more rapidly downward, than upward on the solid nucleus."—*Phil. Trans. Roy. Soc.*, January 17, 1839, vol. cxxix, pp. 381–384.

largely compensated by the frictional heat continually generated by the tidal waves in the superior layers.*

Indeed we may venture the opinion that the ultimate refrigeration of a body like the earth must require a period of time whose curve would be almost hyperbolic to the asymptote; and that the entire solidification of a planet has not yet occurred in the solar system. Even our Moon, with a mass less than one-eightieth of our own, and with an exterior cooled probably below the freezing point of mercury, may still be very hot if not partially molten within.†

The volumetric contraction of our globe since the formation of its crust may therefore be regarded as practically very insignificant. On the other hand if we attempt to form an idea of the amount of average shortening of the strata in a given zone about the earth, from the amount of corrugation and displacement observed (assuming from the analogy of submarine contours that a corresponding degree of compression exists throughout the ocean beds), an allowance of one-eleventh, or of a primitive excess by one-tenth of the present girth, would probably not be regarded by the geologist as an undue assumption. Professor Alphonse Favre of Geneva in detailing some experiments to reproduce by lateral compression in layers of tenacious clay, the plications of strata, has estimated a linear contraction of one-third, as exhibited by portions of the Swiss Alps.‡ Our own Appalachians and western Sierras probably indicate as great an amount of shortening.

But an expansion of the circumference of a sphere by one-tenth, involves an increase of its volume of one-third; of course a wholly inadmissible assumption on any hypothesis of the formation of a crust. The conception of a gaseous or vaporous *intumescence* of a body like the earth may be dismissed at once, as simply opposed to physical possibility.§

* Mr. George H. Darwin has estimated that the internal heat developed by "the lengthening of the sidereal day from 5h. 36m. to 23h. 56m. would be sufficient (if applied all at once) to heat the whole mass about 3000° F., supposing the earth to have the specific heat of iron."—*Phil. Trans. Roy. Soc.*, Dec. 19, 1878, vol. clxx, pp. 495 and 535. And in a subsequent memoir, computing this past transformation of energy as equal to thirteen and a half times the whole remaining kinetic energy of the present rotation, he arrives at the somewhat startling conclusion: "Thus it appears that at the present rate of loss, the internal friction gives a supply of heat for 3560 million years." (Same volume, pp. 561 and 592.) He infers however that only about one-fiftieth of the present downward increase of temperature is referable to the past internal friction.

† This is of course in very strong antagonism to the conclusions reached by Sir William Thomson's elaborate calculations.

‡ La Nature, Sept. 28, 1878, vol. vi, part ii, pp. 278–283. A full abstract of the paper is given in Nature, Dec. 5, 1878, vol. xix, pp. 103–106.

§ This suggestion is broached by Mr. Fisher in his Physics of the Earth's Crust, and appears to be in some sense maintained in a recent German work (not seen by the present writer), on Terrestrial Physics (1884), by Von Siegmund Günther.

What solution of this seeming paradox have geologists to offer, after the demolition of the "contraction hypothesis?" As yet none has appeared. It would seem then that the problem must be attacked from a different quarter.

For a century it had been observed by astronomers that the moon's path exhibited a small acceleration (amounting to about 10″ or 11″ in a century), not accounted for by theory. Laplace investigating the problem, showed that as a result of the secular diminution of the earth's orbital eccentricity, there should be an acceleration of the moon of about 10″ in a century. And he announced it as established from a computation of the ancient eclipses that our "sidereal day has not changed by so much as one-hundredth of a second since the time of Hipparchus."*

That remarkable man, Dr. Julius R. Mayer of Heilbronn, in his very original essay on "Celestial Dynamics" published in 1848, showed that as the oscillations of the pendulum require for their maintenance a constant expenditure of power, so equally do the oscillations of the ocean tidal wave. "The moving waters rub against each other, against the shore, and against the atmosphere; and thus meeting constantly with resistance, would soon come to rest, if a *vis viva* did not exist, competent to overcome these obstacles. This *vis viva* is the rotation of the earth on its axis; and the diminution and final exhaustion thereof will be a consequence of such action. *The tidal wave causes a diminution of the velocity of the rotation of the earth.*" In the next chapter, discussing the consequence of Laplace's demonstration of the constancy of the length of the day in historic times, Mayer continued: "This result, as important as it was convenient for astronomy, was nevertheless of a nature to create some difficulties for the physicist. With apparently good reason it was concluded that if the velocity of rotation had remained constant, the volume of the earth could have undergone no change [by loss of heat]. . . . The earth's radius measures 6,369,800 meters, and therefore its length ought not to have diminished more than 15 centimeters [6 inches] in 25 centuries. . . . Considering what is known about the expansion and contraction of solids and liquids by heat and cold, we arrive at the conclusion that for a diminution of one degree [C.] in temperature, the linear contraction of the earth cannot well be less than the hundred-thousandth part. . . . The reason why in spite of this accelerating cause, the length of the day has nevertheless remained constant since the most ancient times, must be attributed to an opposite retarding action. This consists in the attraction of the sun and moon on the liquid parts of the earth's surface, as explained in the last chapter. According to the cal-

* Sir John Herschel's Outlines of Astronomy, chap. xviii, sec. 908. (See Laplace's Traité de Mécanique Céleste; tom. ii, liv. 5, and tom. iii, liv. 7.)

culations of this chapter, the retarding pressure of the tides against the earth's rotation, would cause—during the lapse of 2,500 years—a sidereal day to be lengthened to the extent of one-sixteenth of a second. As the length of the day however has remained constant, the cooling effect of the earth during the same period of time, must have shortened the day one-sixteenth of a second. A diminution of the earth's radius to the amount of $4\frac{1}{2}$ meters in 2,500 years, corresponds to this effect. Hence in the course of the last 25 centuries, the temperature of the whole mass of the earth must have decreased one-fourteenth of a degree [C.]."* In this ingenious adjustment, the eminent philosopher doubtless greatly over-estimated the earth's cooling and consequent contraction, in order to compromise the "difficulties of the physicist" with the authority of the astronomer.

In 1853, our colleague Mr. William Ferrel in a paper "On the effect of the Sun and Moon upon the rotatory motion of the Earth," independently undertook the mathematical investigation: the very name of Mayer being then (it is scarcely necessary to state) as wholly unknown in this country as it was in England. In this important paper it is shown that external forces of the second order—neglected by Laplace as producing no sensible effect upon the rotation period, are—though very small—decidedly appreciable, and "must, if not counteracted by some other effect, produce a sensible variation in the earth's rotatory motion." Assuming a lunar ocean tide of two feet, with a lag of two hours, Mr. Ferrel estimated an equatorial retardation of 37 miles per century, increased by the solar tide to 44 miles per century, which would give an apparent lunar acceleration of $1'\,24''$ in a century. "As no such acceleration has been observed—above what is accounted for otherwise, the rotatory motion of the earth must be nearly uniform; and the above effect of the sun and moon must be accounted for by the gradual contraction of the earth through a loss of temperature." This would require a subsidence of the surface, or a reduction of the earth's radius (supposing the contraction equable through the mass), of about one foot per century.†

In the same year, a few months earlier, Mr. John C. Adams in a careful memoir "On the secular variation of the moon's

* L. E. D. Phil. Mag., May and June, 1863, vol. xxv, pp. 403, and 423–427. Also Am. Jour. Sci., 1864, vol. xxxviii, pp. 398, and 409–413.

† Gould's Astronom. Jour., Dec. 8, 1883, vol. iii, pp. 138–140. In this paper (p. 141), Mr. Ferrel shows that the similar retarding effect of the earth upon a primitive rotation of the moon (supposed originally fluid) must be 562 times greater than the converse earth retardation; and that this therefore sufficiently explains the present reduction of the moon's rotation to the period of a lunar month. This is believed to be the first suggestion of the now accepted cause of this peculiarity of the moon. Mr. Ferrel's paper is dated September, 1853. A similar explanation was offered by Helmholtz in the following year.

mean motion," pointed out an incompleteness in Laplace's investigation by the neglect of a certain series of terms; and he showed that the effect of this series would be to reduce the actual acceleration due to the diminution of eccentricity in the earth's orbit to about 6″; leaving the residual discrepancy of 4″ or 5″ of acceleration (as compared with the positions indicated from the ancient eclipses), to be still accounted for.*

Herman Helmholtz, in a lecture on the "Interaction of Natural Forces," delivered at Königsberg, Feb. 7, 1854 (without knowledge of Adams' work), pointed out to his hearers that "The motions of the tides, however, produce friction; all friction destroys *vis viva*, and the loss in this case can only affect the *vis viva* of the planetary system. We come thereby to the unavoidable conclusion that every tide—although with indefinite slowness, still with certainty—diminishes the store of mechanical force of the system; and as a consequence of this the rotation of the planets around their axes must become more slow."†

Charles Delaunay, who with fine analytic skill had for several years been engaged on the theory of the moon, in 1859 undertook a careful examination of the results obtained by Mr. Adams, which impeached the accuracy of his great countryman, Laplace; and in a memoir "On the secular acceleration of the mean movement of the Moon," presented to the French Academy, he fully confirmed the bold criticism of Mr. Adams.‡ And he affirmed that the necessary elongation of the day by tidal retardation is quite competent to account for the apparent residual acceleration of the moon.

In 1862 the eminent mathematician, Arthur Cayley, independently investigated the problem; and in a paper "On the secular acceleration of the Moon's mean motion," he also corroborated the correctness of Mr. Adams' great discovery.§

In 1864, eleven years after his first suggestive memoir, Mr. Ferrel returned to the subject of the elongation of our day. In a "Note on the influence of the Tides in causing an apparent secular Acceleration of the Moon's mean motion," he remarks: "At the time of the original publication of Mayer's paper, and also at the time of my own, Laplace's result of

* Phil. Trans. Roy. Soc., June 16, 1853, vol. cxliii, pp. 397–406. Also Monthly Notices, Roy. Astr. Soc., 1853, vol. xiv, pp. 59–62. And Reply to Objections, M. N., 1860, vol. xx, pp. 225–240, and 279–280.

† L. E. D. Phil. Mag., June, 1856, vol. xi, p. 513. Also Am. Jour. Sci., 1857, vol. xxiv, p. 212.

‡ Comptes Rendus Acad. Sci., Jan. 17 and April 25, 1859, vol. xlviii, pp. 137, 138, and 817–827. Also numerous papers on the subject in succeeding volumes. Especially a paper "On the apparent acceleration of the mean motion of the Moon, due to the actions of the Sun and Moon on the waters of the sea."—Comptes Rendus, 1866, vol. xlii, pp. 197–200, 575–579, 704–707, etc.

§ Monthly Notices, Roy. Astr. Soc., 1862, vol. xxii, pp. 171–230.

about 11′ per century for the amount of acceleration of the moon's mean motion arising from a secular change of the eccentricity of the earth's orbit was supposed to be correct; but since that time it is known that Adams and Delaunay, and also quite recently, Mr. Cayley, have all obtained less than 6″ for the amount of secular acceleration per century. This determination, compared with the most reliable determination of the acceleration deduced from the discussion of the recorded observations of ancient eclipses, leaves about 6″ to be accounted for by tidal action or some other *unknown cause.*" He then proceeds to state that Mayer's hypothesis of a one-and-a-quarter hour retardation, and his own of a two-hour retardation of the tidal wave, "are both much too great to be reasonable hypotheses, or to be necessary to account for the balance of the moon's secular acceleration over and above the late determination." And he concludes "that tidal action is adequate to account for 6″ of secular acceleration upon a very reasonable and probable hypothesis with regard to the magnitude and retardation of the tidal wave by friction after making due allowance for the effect of a probable contraction of the earth's volume."*

In 1867, Prof. William Thomson, discussing the same subject says: "The tides must tend to diminish the angular velocity of the earth's rotation, and this tendency is not counterbalanced to more than a very minute degree by the tendency to acceleration which results from secular cooling and shrinkage."†

This brief historic summary has been introduced to emphasize the strengthening conviction among physicists of the necessary variability of that standard unit of time, which to the astronomers had seemed the most permanent constant of our solar system. And these references serve to establish beyond a reasonable doubt the remarkable fact that our present day is considerably longer than the day of early geologic times. For nearly two centuries has the moon been seen in advance of its periodic position—to the small amount (as estimated from ancient eclipses), of about ten or eleven seconds of angle per century. Of this relative displacement about one-half is now known to be due to the actual falling behind of the terrestrial observer, and the other half to Laplace's great discovery—the influence of the diminishing eccentricity of the telluric orbit.‡

Mr. George H. Darwin (as is well known) has in recent years been engaged in elaborate investigations of this subject, and he has drawn some important consequences from his discussion.

* Proceedings Am. Acad. Arts and Sci., Dec. 13, 1864, vol. vi, pp. 379–383.

† Thomson and Tait's Natural Philosophy, 1867, vol. i, Sec. 830, p. 686.

‡ An influence which must reach its inferior limit in about one precession period.

He says: "As we look at the whole series of changes from the remote past, the ellipticity of figure of the earth must have been continually diminishing, and thus the polar regions must have been ever rising, and the equatorial ones falling; but as the ocean always followed these changes they might quite well have left no geological traces. The tides must have been very much more frequent and much larger, and accordingly the rate of oceanic denudation much accelerated. The more rapid alternations of day and night would probably lead to more sudden and violent storms, and the increased rotation of the earth would augment the violence of the trade winds, which in their turn would affect oceanic currents. Thus there would result an acceleration of geological action."*

In this suggestive retrospect one important physical condition, however—which probably *has* "left its geological traces" on our globe—does not appear to have attracted the author's attention. A shell formed upon the spinning earth when its oblateness was considerably greater than at present obviously could not fit the spheroid as the ellipticity of its meridians diminished. Not only would the crust be quite sensibly too large, as a whole, but especially would its equatorial girth have to be notably reduced.

If in imagination we may carry back the formation of a consistent crust (of some few miles thickness) to an epoch when the rotation of our planet was at four times its present rate—that is, when the day measured but six of our hours,—the equatorial radius (assuming a true ellipsoid of revolution, and neglecting the small amount of contraction by loss of heat), would have been about one-tenth greater than it now is, or 4359 miles; and the polar radius about one-sixth less, or 3291 miles. In other words, the poles would have been about 658 miles nearer the center of the earth than they are at present, and the equatorial protuberance about 396 miles higher than at present.

With an equatorial shell one-tenth greater in circumference than the present dimensions, it is evident that from the very slow but never ceasing contraction due to diminution of rotatory motion, this crust would be subject to an unremitting stress of lateral compression as relentless as that from the old hypothetic shrinkage of volume by reduction of temperature. Is it not precisely this *morphologic* contraction whose effects and records are everywhere apparent in the crumpling of the earth's crust? Here is a true cause: here is a sufficient cause: here is a necessary cause—so importunate that it cannot readily be dispensed with or explained away.

An objection to this hypothesis, based on the supposed

* Phil. Trans. Roy. Soc., December 19, 1878, vol. clxx, p. 532.

solidification of our planet since its present form has been reached, requires a notice here, rather from the character of its source, than from its own intrinsic force or importance.

Professor William Thomson, adopting Mr. Adams' estimate of tidal retardation, thus argues: "If the rate of retardation had been uniform since ten million centuries back, the earth must have been then rotating faster by one-seventh than at present, and the centrifugal force greater in the proportion of 64 to 49. If the consolidation took place then, or earlier, the ellipticity of the upper layers of equal density must have been $\frac{1}{230}$ instead of about $\frac{1}{300}$ as it certainly now is. It is impossible to escape the conclusion that the date of the consolidation is considerably more recent than a thousand million years ago.*

Professor Peter G. Tait (as if not to be outdone), is still more emphatic and positive in his enunciation. He says: "It being thus established that the rate of rotation of the earth is constantly becoming slower the question comes: How long ago must it have solidified in order that it might have the particular amount of polar flattening which it shows at present? Suppose, for instance, it had not consolidated less than a thousand million years ago. Calculation shows us that at that time, on the most moderate computation, it must have been rotating at least twice as fast as it is now rotating. That is to say, the day must have been 12 hours long instead of 24. Now if that had been the case, and the earth still fluid throughout, or even pasty, that double rate of rotation would have produced four times as great centrifugal force at the equator as at present, and the flattening of the earth at the poles and the bulging at the equator would have been much greater than we find them to be. We say then, that because the earth is so little flattened it must have been rotating at very nearly the same rate as it is now rotating, when it became solid. Therefore, as the rate of rotation is undoubtedly becoming slower and slower, it cannot have been many millions of years back when it became solid, else it would have solidified into something very much flatter than we find it."†

The supposition that a granite mountain or equatorial protuberance 400 miles high or 100 miles high, could for a moment support itself would hardly be entertained by a practical civil engineer.‡

It is now nearly forty years since Herbert Spencer, with a juster physical insight, contended and satisfactorily showed that a solid earth (of any shape) would assume the oblate

* Nat. Philos., 1867, vol. i, Sec. 830, p. 687.

† Lect. on recent advances in Phys. Sci., 1876, lect. vii, pp. 173, 174.

‡ The limiting modulus of height of a granite pyramid (equaling one side of its square base), is somewhat less than eleven miles.

spheroidal form due to its rate of rotation, as certainly and promptly as if it were liquid.*

Granting then that the earth were solidified with an oblateness such as has just been assumed, its present dimensions—and probably its present topography—would not be very different from what we now find them.

But another very serious error in the estimates of both these quotations should not be overlooked. While it is quite impossible to calculate the time when the equatorial radius was one-tenth greater than at present, it is very certain that any estimates based on the existing retardation from the ocean tides, must be utterly fallacious. Not only has this tidal action been continually decreasing (by some small exponent) from the earliest times, but the bodily tides of the liquid or solid earth have undoubtedly played a far greater part than the superficial ocean tides—in the absorption and destruction of its rotatory momentum.† It is not at all probable therefore that it has required "a thousand million years" to effect the present equatorial contraction of one-eleventh from the former diameter of 8,718 miles.

The much debated question of the probable degree of rigidity of our planet is therefore quite irrelevant to the problem before us. And yet with so much confidence and persistence has the pet-hypothesis of its entire solidity been maintained,—the temptation is strong to waste upon it a collateral glance.

Professor William Thomson contends: "Had the solid part of the earth so little rigidity as to allow it to yield in its own figure very nearly as much as if it were fluid, there would be very nearly nothing of what we call tides, that is to say, rise and fall of the sea relatively to the land; but sea and land together would rise and fall a few feet every twelve lunar hours. This would (as we shall see) be the case if the geological hypothesis of a thin crust were true. The actual phenomena

* "However great in a given portion of matter may be the excess of the form-preserving force over the form-destroying force, it is clear that if during augmentation of bulk the form-preserving force increases only as the *squares* of the dimensions, while the form-destroying force increases as their *cubes*, the first must in time be overtaken and exceeded by the last; and when this occurs the matter will be fractured and re-arranged in obedience to the form-destroying force."—*Herbert Spencer.* And the author estimates that "the most tenacious substance with which we are acquainted—when subjected to the same forces that are acting upon the earth's crust—would exceed the limit of self-support determined by the above law, before it attained the thousand-millionth of the earth's bulk."—*L. E. D. Phil. Mag.*, March, 1847, vol. xxx, pp. 194–196.

† Mr. G. H. Darwin remarks: "Whatever may be thought of the theory of the viscosity of the earth and of the large speculations to which it has given rise, the fact remains that nearly all the effects which have been attributed to the action of bodily tides would also follow (though probably at a less rapid rate), from the influence of oceanic tides on a rigid nucleus."—*Phil. Trans. Roy. Soc.*, Dec. 19, 1878: vol. clxx, p. 538.

of tides therefore give a secure contradiction to that hypothesis."*

That a siliceous crust of 20 miles average thickness, and an overlying aqueous ocean of three miles average depth, should have (as required by the argument) so equal a coefficient of mobility, that sea and land could thus "together rise and fall," might well be pronounced incredible. Professor Thomson himself proceeds to very seriously damage his "secure contradiction" of "the geological hypothesis," by adding immediately after the passage just quoted: "We shall see indeed presently that even a continuous solid globe of the same mass and diameter as the earth would if homogeneous and of the same rigidity as glass, or as steel, yield in its shape to the tidal influence, three-fifths as much, or one-third as much, as a perfectly fluid globe."†

When we have learned the elements of depth, inertia and fluidity of the disturbed layers beneath the geologic crust, then and not till then may the beginnings of a mathematical theory of the telluric tides be attempted; and some plausible measure of their "lagging" be suggested. But in our profound and insuperable ignorance of these interior conditions determining the magnitude and direction of the tidal crests, it is idle for the mathematician (with whatever array of formidable differential equations) to gravely assure us that "no very considerable portion of the interior of the earth can even distantly approach the fluid state."‡

It is in fact quite incontrovertible, that whether the geologic crust have for its content a glowing lava, or Thomson's ideal steel, it is equally subject to lunar and solar tides fully comparable to the more apparent ocean tides. This last surviving argument for solidity, from the theory of the tides, should therefore be dismissed as being no less futile than the abandoned argument, from the theory of precession.§

* Nat. Philos., 1867, vol. i, sect. 833, p. 690. Retained in the new edition.

† Same work; sect. 833, pp. 690, 691, and sect. 834, pp. 691–694.

‡ Mr. George H. Darwin in a very abstract memoir: "On the bodily tides of viscous and semi-elastic spheroids, and on the Ocean Tides upon a yielding nucleus," read before the Royal Society of London, May 23, 1878, arrives at the conclusion: "Unless the viscosity were very much larger than that of pitch, the viscous sphere would comport itself sensibly like a perfect fluid, and the ocean tides would be quite insignificant. It follows therefore that no very considerable portion of the interior of the earth can even distantly approach the fluid state."—*Phil. Trans. Roy. Soc.*, vol. clxx, p. 28.

§ Professor Thomson, who in 1862 had re-enforced and exaggerated the Hopkins argument, by maintaining that the earth's defect (by deformation) from the theoretical amount of precession, is "much smaller for instance than it would be if its effective rigidity were no more than the rigidity of steel," in consequence of an oral discussion with our colleague Professor S. Newcomb, in 1876, with a frankness worthy of all praise, made a full surrender of the position, in his address before the mathematical and physical section of the British Association: Report Brit. Assoc., Sept., 1876, vol. xlvi, part II, pp. 1–12), and has since con-

Returning to the consideration of the varying oblateness of the terrestrial ellipsoid, and its geological consequences, we naturally inquire what indications remain to corroborate or to impair our hypothesis. Could we ascertain the approximate amount of lateral excess of shell by its corrugations, say for each parallel zone of ten degrees, these enlarged circumferences divided by the cosine of the latitude would give us the variations (if any) of envelope, throughout a given meridian. This is of course entirely beyond our reach. We may, however, infer that the altitudes of mountain ranges should bear some rude proportion to the general amount of crumpling in neighboring districts; and that at least some approximate indication might thus be afforded or suggested. We might expect that circumpolar regions should be free from mountains or plications; and that the inter-tropical region should contain the highest elevations.

Dr. Arnold Guyot, in his excellent summary of the Physics of the Globe, prepared for Johnson's Cyclopædia, gives the following characteristic sketch: "In the New World therefore the highest lands are piled up in the southwest, in the Old World in the southeast. . . . On the whole, the reliefs begin with the vast low plains around the polar circle, and go on increasing from the shores of the Arctic Ocean toward the tropical regions. The highest elevations however are not found at the equator, but north of the Tropic of Cancer in the Old World, in the Himalayas 28° N., and north of the Tropic of Capricorn in the New World, in the Andes of Bolivia 16° S."*

The same eminent and conscientious physiographer, in his paper on the Ocean, remarks: "On the whole, the ocean beds become less deep toward the north pole, just as the lands become lower toward the same region."†

In this significant epitome—presented certainly with no view of supporting an hypothesis, we have perhaps as striking a suggestion of greater oblateness in former times as we could expect to find preserved to us. The curious circumstance that the highest lands on either hemisphere are found much nearer to the Tropics than to the Equator, it must be admitted, is difficult of explanation. Theoretically we should expect no sensible differences throughout the inter-tropic zone. We may however remember that the tides of the comparatively thin and yielding crust exert their greatest influence on the tropical zones; being for the larger portion of the year very near to

ceded "that a perfectly fluid spheroid has a precession scarcely differing from that of a perfectly rigid one."—Mr. Darwin, in *Phil. Trans. Roy. Soc.*, Dec. 19, 1878, vol. clxx, p. 525.

* Johnson's Cyclopædia, 1875: art. "Earth," vol. i, p. 1455.

† Same work, 1877, art. "Ocean," vol. iii, p. 918.

those circles; while the equatorial tides—during the equinoctial periods—occur when the sun (and the mean lunar plane) have their most rapid change of declination. If it be said that a tidal elevation of two or three feet is too utterly insignificant a space, and a cycle of half a year too utterly insignificant a time to affect the vast erections of secular progression, the reply is that this minute relaxation of particular zones—unceasingly repeated and continued—may well facilitate the slow but gathering movements of the crust from its strong compression, and may even to some extent give direction to their energies. As a concurrent though perhaps unimportant circumstance, the fact may be mentioned that while the northern tropic encounters 35 per cent of dry land, and the southern tropic nearly 25 per cent, the equator embraces but 21 per cent of land.

George Darwin, discussing the differences of tidal energy and lagging at different latitudes (in a viscous spheroid), by reason of which the retarding influence is greatest at the equatorial zone, suggests the possibility of this tendency causing a westward drift of mountain folds and continents in the lower latitudes (as compared with polar regions) and remarks: "There can be little doubt that on the whole, the highest mountains are equatorial, and that the general trend of the great continents is north and south in those regions. The theoretical directions of coast line are not so well marked in parts removed from the equator."* Here again, the striking significance of this physiographic generalization appears to be strangely overlooked.

It has been an occasion of some surprise that in all the geological literature to which I have had access, I have been able to find but a single allusion to the hypothesis here advanced in explanation of the palpable tangential compression of the earth's exterior strata.

This one allusion is given in Mr. O. Fisher's interesting work on the "Physics of the Earth's Crust," and is by him referred to, only to be dismissed in a brief paragraph. His reference is as follows: "The friction of the tides—whether oceanic or bodily—must necessarily have diminished the rotational velocity, and lessened the oblateness. The parts of the crust about the poles will consequently have been subjected to stretching, and those about the equator to compression. There is however no apparent reason immediately to connect the inequalities with this cause, for the continents do not occupy an equatorial belt—as they would do under this hypothesis, nor have the polar regions been free from the compression which all continental areas have experienced."†

* Phil. Trans. Roy. Soc., Dec. 19, 1878, vol. clxx, p. 589.

† Physics of the Earth's Crust, 8vo, Lond., 1881, chap. xiv, p. 183.

The expansion or stretching of the polar regions—referred to, though probably less than Mr. Fisher would imagine, would be attended with no dislocations, and would leave no traces. A crust of ten or twenty miles depth pressing upon its interior bed with a weight of five thousand or ten thousand tons to the square foot, would flow (excepting its exterior film of a mile or two),—on any relief of lateral pressure—as quickly and as uniformly as so much plastic clay.

To Mr. Fisher therefore belongs (so far as I am aware) the credit of the first suggestion of a possible connection between variation of oblateness and mountain-building;—even though by him discarded. He does indeed on the following page recur to the topic; and admitting that this variation *ought* to have produced some appreciable effect, he suggests that this apparent want of relation between the occurrence and the phenomena, favors the idea of a change of latitudes.

So strongly impressed is the writer with the inevitable operation and potency of this unquestioned retardation of rotation, that were all traces of any differential action masked and obliterated, he would still hold to it as the one efficient cause (alone—as yet suggested) to account for the prominent constriction of the crust—displayed in every land. But the differential traces of oblateness have *not* been obliterated;—masked though they may be to some extent, by other perturbations.

The suggestion of a change of axis, is one which will be entertained by the physicist with extreme hesitancy and caution. Professor William Thomson indeed has stated that "the axis of maximum inertia, and axis of rotation—always very near one another—may have been in ancient times very far from their present geographical position; and may have gradually shifted through 10, 20, 30, 40 or more degrees, without at any time any perceptible sudden disturbance of either land or water."* George Darwin also has admitted that if the earth be not solid, "As in successive periods the continents may have risen and fallen, the pole may have worked its way in a devious course some 10° or 15° away from its geographical position at consolidation; or may have made an excursion of smaller amount, and have returned to its old position."†

That under the continued stresses of precession and nutation—there should have been a slight slipping of the crust on its fluid interior, is not improbable. The poles of the shell in such case might describe a small spiral about the "axis of maximum inertia," but could probably never diverge therefrom more than a very few degrees. Nor could such gyration

* Report Brit. Assoc., 1876, vol. xlvi, part ii, p. 11.

† "On the Influence of Geological changes on the Earth's Axis of Rotation." Phil. Trans. Roy. Soc., Nov. 23, 1876, vol. clxvii, p. 305.

in any degree affect the sidereal axis of rotation—or its angle of obliquity with the plane of the ecliptic. Any such *geographical* change of the axis therefore as admitted by Thomson and Darwin, could only be a shifting of the mass of the earth (so to speak) upon an axis fixed in angular direction, with a corresponding shifting within its substance of the equatorial plane of oblateness.

Were we at liberty to imagine a translocation of the northern geographical pole—in the remote past, as far as to the arctic circle near Bering's Straits, the equatorial region would nearly correspond with Guyot's line of demarkation of the three northern—from the three southern continents.* This re-arrangement would also bring the highest portions of the South American Andes and of the Asiatic Himalayas much nearer to the equator, and the remarkable elevations of southern Europe—the Pyrenees, the Alps and the Caucasus (now all near the 45th degree of latitude), to the northern tropic. This, however, is mere speculation, for which no scientific warrant can at present be given.

An objection urged with considerable force by Captain Dutton against the hypothesis of the earth's contraction through secular cooling, should not be overlooked since it lies equally against the supposition here presented, and indeed against any hypothesis of general contraction. He remarks: "The determination of plications to particular localities presents difficulties in the way of the contractional hypothesis which have been underrated. . . . . The tendency of corrugation to occur mainly along certain belts with series of parallel folds, is not explained by assuming that these localities are regions of weakness. For a shrinkage of the nucleus would throw each elementary portion of the crust into a state of strain by the action of forces in all directions within its own tangent plane. . . . . The plications of the Paleozoic rocks do not conform, either in Europe or America, to the consequences here affirmed. These disturbances are localized in long and rather narrow belts, and if they truly represent contraction on certain great circles, then such contraction must have been enormous in arcs perpendicular to the axes of plication, and very little in arcs parallel thereto. Still more discordant is the contractional hypothesis with the Tertiary plications. From Cape Horn to the Bering's Sea is a continuous belt, very narrow for most of the distance, but extremely disturbed throughout."†

* Professor Guyot, referring to the general direction of the line of separation marked out by the Caribbean and Mediterranean Seas, with their adjacent isthmuses, remarks: "These regions are parts of a broad transverse band whose position can be traced from Bering's Straits as a center, with a meridian arc of 80° radius, and which we would call the *central zone of fracture*."—Johnson's Cyclopædia, 1875, vol. i, pp. 1449, 1450.

† Am. Jour. Sci., August, 1874, vol. viii, pp. 121, 122.

It cannot be denied that the difficulty here set forth is a very puzzling one. Especially inexplicable appears the instance last referred to—of an unbroken range of mountain foldings extending over more than a third of the globe's circumference. While the *force* at the command of the rotating planet is abundantly sufficient to accomplish the result evidently some supplementary considerations are requsite to give the observed *direction* to this force. Beyond all question, the surface of the earth *has* been subjected to a compressing stress of tremendous energy. To account for the stress is one problem; to account for its resultants is another and probably much more complicated one.

The mere mechanical difficulty, however, of transmitting stresses through comparatively undisturbed areas of hundreds of miles of a flexible, friable, and practically plastic crust—with a large coefficient of viscous friction beneath—is not so formidable as might at first sight appear. It must be borne in mind that the pressures derived from an action so slow as from century to century to be scarcely sensible, are of an order of very great intensity, but of very small quantity. Under the continued urgency of rapidly revolving tidal waves (though also of a very minute order), there does not seem any improbability in the supposition that with the long time element such stresses may gradually be equalized or transmitted by what might be termed a process of "conduction" almost indefinitely.

From various considerations we may infer that in all geological ages the progress of elevation has been in excess of that of degradation by erosion; that in all ages mountain building has been at a maximum; that is, the uplifted heights have been the greatest which the average thickness of the crust at the time was capable of supporting; so that the former has been a constant function of the latter, the ratio being probably not far from one-fifth.

We may also infer that this increasing maximum of elevation must now have practically reached its limit, since both the processes of equatorial contraction and of internal temperature reduction are going on with extreme and lengthening slowness; and the whole remaining subsidence of the inter-tropical oblateness cannot exceed five miles, during the vast ages in which the earth's rotation shall be entirely arrested.

Looking back through the long vista of the unmeasured past we behold in imagination our planet in its early youth, with its expanded tropical surface as yet unmarked by wrinkles, endowed with superfluity of rotary activity, and subject to far more energetic tides and winds and tempests than we know at present. Its primitive and lowly organisms with

as yet unspecialized sensations, accustomed to a dusky day of a few brief hours succeeded by as short a night. The moon—perhaps much nearer and of ampler but paler disk—speeding through its phases in the quick period of a week or less. The sun—of enormously greater volume than we behold it—diffusing its hazy beams, and probably expending a much smaller amount of radiant light and heat upon the flat and dreary face of nature than it now does after many hundred million years of waste.* Such is the outline of early meteorological conditions which the geologist should take into his account when theorizing on the grand dynamics of his science.

But of that primeval period of fleeting days and of shortened axis, perhaps the only physical record and memorial left us is the wide array of distorted crumplings and ruptured foldings (culminating apparently in the lower or middle latitudes) which have formed the needful condition and environment for man's advancement, and which have never ceased to excite the wonder and admiration of his observant and inquiring intelligence.

ART. XXXIII.—*The Old Tertiary of the Southwest;* by E. W. HILGARD.

IN an article published in the June and July numbers of this Journal, Dr. Otto Meyer undertakes to show, not only that numerous supposed species of fossils heretofore described from the Tertiary of the southwestern States should be canceled as being mere variations of no specific value, but also that there is good reason to suppose that the stratigraphic succession of the several stages, as heretofore understood and accepted, is incorrect and actually requires to be turned upside down. As regards the former part of his thesis I thoroughly agree with him, if not in detail at least in the general issue. As regards his second point, it is simply incorrect; and it is difficult to understand how, if Dr. Meyer took the pains to do more than look over the lists of fossils in my report on the geology of Mississippi, he could entertain such a proposition for a moment. The only explanation of his error can be found in the fact, evident from the whole of his article, that he is unacquainted with the methods of field geologists, and imagines that the paleontologist is the final arbiter in all questions of geological age. There was a time when this idea was current even among geologists; but at least on this side of the Atlantic it has for a number of years counted among the "*überwundene Standpunkte.*"

* It is well known to physicists that radiation is not in proportion to temperature. The "lime-light" radiates a far greater amount both of heat and of light than the simple oxy-hydrogen flame at a higher temperature.

Dr. Meyer commits a fundamental error of judgment in another matter, namely, in the assumption that after Lyell, Conrad and Tuomey had issued their (well grounded) dicta in regard to the succession of the Tertiary stages, those following them in the investigation of the subject calmly took these things for granted, and made their observations conform to "the masters' words." Here again, Dr. Meyer assumes a state of affairs which not long ago was widely prevalent in the old world, but has not, within my recollection, been a fault of American observers. On the contrary, young men have rather tended to distinguish themselves by making startling discoveries of mistakes in the work of their predecessors, and have left nothing unchallenged and unverified. Contrary to Dr. Meyer's expressed opinion of my method of work in Mississippi (see his paper in July number, page 65), I was even then sufficiently Americanized to subject every point of my predecessors' work and conclusions to the closest and most elaborate scrutiny, as he would have found out had he done me the honor to study my report. Hence I have no comment to make on his historical presentation of the growth of opinions, except that those opinions served me merely as convenient working hypotheses. But I differ from him more fundamentally in his sweeping statement (*Ibid.*), that "only a competent and careful examination of the fossils could indicate the relations of the Old-tertiary strata of Mississippi," and that I "studied this Tertiary paleontology very little." I had not time, it is true, for a thorough study of *all* the forms occurring in the several stages; and it is also true that under pressure of work, I "transferred the making of the *lists* of fossils to Prof. W. D. Moore." But every one of these fossils had been collected by myself personally, and in so doing I had acquired a very competent knowledge of the leading fossils of each of the stages; it had also convinced me of the fact that Conrad had made a large number of spurious species, and that the several stages are intimately interconnected by community of species from Claiborne to Vicksburg. This conviction I have repeatedly expressed in my Mississippi report, and it is emphasized by the list of fossils from the "Red Bluff" locality, which shows an obvious transition from the Jackson to the Vicksburg fauna. But it, as well as a great many other observations, also emphasized the extreme *localization* of certain fossils and groups of fossils; a circumstance easily accounted for by the shallowness of the depositing sea, evidenced not only in the materials and littoral fauna, but in the constant recurrence of brackish and lignitic facies where, stratigraphically, the continuation of marine beds was to be expected. This made me extremely cautious in relying on any single or few fossils for the identifi-

cation of stages in distant localities; and similar caution would have prevented Dr. Meyer from coming to many false conjectures on insufficient premises.

It is difficult to characterize in terms altogether courteous Dr. Meyer's supposititious account of my mode of constructing my geological map of the formations of Mississippi (*Ibid.*); for he had before him, in print, the record of the localities through which the lines were drawn, and in stating that I made the acute southward curve of the Vicksburg belt around Jackson simply by way of getting out of a difficulty engendered by a hasty adoption of my predecessors' views, he states what the printed record shows to be false. This curve is necessitated by the occurrence of characteristic Vicksburg fossils in the following localities: Brownsville, Marshall's Quarry, Byram, German Berry's (then Monterey Post Office), Brandon; and no other delineation is possible. How is it that with these observations plainly in the text before him, and with the results of his own examinations at Jackson itself, it did not occur to him to walk down Pearl River nine miles, as I did, to see the Jackson strata sinking out of view, to be replaced, first by brackish and more or less lignitiferous deposits, which in their turn sink out of view and are capped by the Vicksburg rocks in their most characteristic development, in a magnificently fossiliferous outcrop two miles long? How does Dr. Meyer expect that in a level or merely rolling country, underlaid by strata having a dip not exceeding ten feet per mile, he will ever see stages separated by strata sixty or seventy feet in thickness exposed in any one outcrop? and in the present case, is any fossil whatsoever needed to establish the order of superposition?

There are at least two other lines of section across the Tertiary belt of Mississippi, in which the order is just as plainly established by stratigraphy alone, independently of fossils, as it is on Pearl River. One of these lines lies along the Yazoo Bluff, from Carrollton via Vicksburg down to Grand Gulf; the other, from Meridian via Enterprise and Quitman down to Winchester, on the Chickasawhay River. The former section has been completely explored between Professor E. A. Smith (then my assistant) and myself; the latter was in the first place traversed by myself, by land, and then, to make assurance doubly sure, and in order to observe details, the entire trip was made in a canoe by Professor George Little, later State Geologist of Georgia and now at the University of Mississippi. In all cases alike, *going down streams flowing to the southward, the strata successively sink below the water's edge as the observer progresses, in the order Claiborne, Jackson, Vicksburg, Grand Gulf,* as identified by their leading fossils. If then, there is any

virtue in geometry, this is the inevitable order of succession from below upward.

But, outside of the State of Mississippi, I can satisfy Dr. Meyer's postulate of "seeing the Vicksburg rocks actually superimposed upon the Jackson strata." I have seen this in Louisiana, on the Bayou Funne Louis, where I have stood on a ledge of Vicksburg limestone showing a southward dip and containing abundance of *Orbitoides*, *Arca Mississippiensis* and *Pecten Poulsoni*, looking down some eighty feet, to northward, upon a level prairie country in which the bones of the Zeugiodon have been plowed up.

Without discussing paleontological details for which in the absence of adequate literature and collections I should now have to rely on memory alone, I must remark that I cannot attach much importance to *Plagiostoma dumosum* as a significant fossil. Of hundreds of localities examined by me in Mississippi, only two have yielded this shell; and both belong to a level intermediate between the Jackson and Vicksburg groups. But no Vicksburg locality has failed to furnish what I have been led to consider the decisive mark of the age, viz: *Arca Mississippiensis;* it is more constant than either the *Orbitoides* or *Pecten Poulsoni*, although the latter is rarely absent. For the Jackson age the most constant fossil is the *Zeuglodon*, bone fragments of which can nearly always be found by diligent search; and besides, an excellent and constant criterion is the presence of *Venericardia planicosta*, which has nowhere been found associated with the characteristic Vicksburg fauna. Through this widely diffused and universally recognized shell, as well as through the almost equally constant *Gastridium vetustum* and *Calyptrophorus velatus* as common fossils, the Jackson fauna connects strikingly with the Claiborne and Buhrstone beds; and I have found this *Venericardia* in the latter, in almost immediate contact with the Upper Cretaceous rocks of North Mississippi. Upon Dr. Meyer's assumption, the Vicksburg beds, void of both of the above types, would actually be intercalated between this oldest post-Cretaceous fauna and the Claiborne and Jackson beds. However, his assumption is abundantly and conclusively disproved by the most direct stratigraphical evidence; which it is to be hoped he himself will undertake to verify before he again ventures to re-classify the Southwestern Tertiary.

ART. XXXIV.—*Remarks on a paper of Dr. Otto Meyer on "Species in the Southern Old-Tertiary;"* by EUGENE A. SMITH.

IN this paper, one of the objects of the author is to show that the relative position of three of the subdivisions of the Tertiary formation, viz: Vicksburg, Jackson and Claiborne (given in descending order), is not the true one, but that it should be Claiborne, Jackson and Vicksburg, with Claiborne at the top and Vicksburg at the bottom.

This conclusion of Dr. Meyer cannot be allowed to pass without comment; for although no geologist who has ever been across the country where these rocks occur in Alabama, could for a moment be in doubt as to their relative position, yet those unacquainted with the facts in the case might be led by Dr. Meyer's paper to doubt the accuracy of the observations of Lyell, Tuomey, Winchell and others.

So far as I am able to make it out, Dr. Meyer's conclusion is based upon two observations, and upon a number of inferences derived from what he thinks should have been the course of evolution of several species of shells.

The observations are these: (1) Conrad found in one of the lower strata of the Claiborne bluff, below the ferruginous sands which bear the greater part of the well known Claiborne fossils, a specimen of *Spondylus dumosus;* and (2) Dr. Meyer himself, found a specimen of an *Orbitoid* in one of the lower strata at Claiborne. I shall say nothing of the possibility of these specimens having been washed down from a higher level, for it is to be supposed that these two observers were not mistaken as to the actual occurrence of the fossils in the rocks mentioned, nor will I refer to the fact that these two observations, so far as they prove anything, merely show that the two species named have a greater range geologically than has usually been assigned to them, for I know from personal observations near St. Stephens and elsewhere, that they occur in great numbers, *above* the Claiborne sands also, but shall confine myself to stratigraphy, since superposition is after all the only absolute test of relative age.

The term White limestone, has been applied in Alabama to a series of calcareous rocks over 200 feet in thickness. A portion of the upper part of this is a chalky rock quite soft and easily cut when freshly dug, and much used as material for building chimneys. This upper part also in places contains great numbers of *Orbitoides Mantelli.* The lower fifty or sixty feet are more clayey, give rise on disintegration to a limy soil which resembles closely the limy soil of the Rotten

limestone of the Cretaceous. This lower part of the limestone contains also numbers of *Spondylus dumosus*, and vertebræ of *Zeuglodon cetoides*. These facts concerning the character and contents of the White limestone I give from personal observations. By many writers the upper part of this limestone has been considered equivalent to the Vicksburg, and the lower, to the Jackson divisions, but for our purposes it will be sufficient to define as above what we term the "White limestone."

Before offering any proof of the relative position of the White limestone and the Claiborne sands, it may be well to call attention to very important omissions of Dr. Meyer in his quotations of Sir Charles Lyell.

In Dr. Meyer's paper, page 61, we find the following remarks on an extract from a letter of Lyell published in this Journal, II, vol. i, pp. 313–315. "In this letter Lyell speaks on several subjects and says, of the Nummulitic limestone, that it had been referred to a pre-Tertiary age, but he considers it even younger than the Claibornean, 'for it occurs on higher places than the Claiborne bluff, a circumstance which, in a region where the stratification is horizontal, would imply a newer deposit, *even if the section near Suggsville, and Clarkesville*, SHOWING SUPERPOSITION *had been less satisfactory*. I did not meet with the limestone in question in the bluff at Claiborne which I have no doubt is owing to the fact that the calcareous strata are cut off at the top before they extend upward into the nummulitic beds.'"

Dr. Meyer omits all the words in italics, and thus carefully excludes the *proof of superposition* which Lyell brings forward; and then charges him with putting forth an hypothesis without proof. Moreover, in allusion to the diagram published by Lyell, in this Journal, II, vol. iv, pages 188–189, showing a section from Bettis' Hill in Clarke County to Claiborne, in which the White limestone is very distinctly represented as occurring at the top of the Claiborne bluff, Dr. Meyer insinuates that Lyell represented the dip, etc., not as he observed it, but as he needed it for his hypothesis. That Sir Charles, in saying that he did not observe at Claiborne the limestone in question, meant the *upper* or *nummulitic part*, is very evident from the figure referred to as well as from the words of Lyell quoted.* Indeed, Lyell's whole description of the relative positions of the various strata and their composition, in the region between the two rivers in the latitude of Claiborne, is remarkably accurate, as any one who is at all familiar with the country cannot fail to see.

Thus, when Dr. Meyer says Lyell has put forward his hy-

* This nummulitic part does, however, actually occur on the hill one mile south of Claiborne, as shown below in § 1.

pothesis of the newer age of the nummulitic limestone without any proof, he does so in contradiction to Lyell's direct and specific statement of what he had actually seen. (This Journal, 1847, pp. 188, 189, 190.) It is because the evidences of superposition are so numerous and unmistakable, that all geologists who have ever examined the country have never questioned it.

I have some sections to present which will remove all doubt, if only we can agree upon what we mean by the terms White limestone, and the Claiborne ferruginous sands. I have already said what I mean by the former. By the latter, I mean the stratum about 15 to 17 feet thick, occurring about midway of the Claiborne bluff (equal to *e* of Meyer's section) consisting of a reddish ferruginous sand full of the fossils described by Lea and Conrad as the Claiborne fossils. This sand is in its appearance and fossil contents unmistakable by any one who has ever seen it once.

The proofs which I wish to offer are of two kinds, viz: (*a*) direct superposition, and (*b*) the geographical position of the outcrop.

a. From direct superposition.

Immediately overlying the red ferruginous sands containing the Claiborne fossils—there are about three feet of ferruginous sands with hard ledges filled with *Scutella Lyelli.* This Scutella bed is indicated by *f* in Meyer's section at Claiborne, and it may be seen at all the localities cited below. Close above this Scutella bed is a White limestone—in places very argillaceous—sometimes sandy and glauconitic, which is demonstrably the lower part of the White limestone series as above limited. This is seen

1. At Claiborne.* Here there are to be seen between 50 and 60 feet of the clayey and glauconitic limestone (*h* and *i* of Meyer's section)—overlying the Claiborne ferruginous sands and the Scutella bed. On going from the plateau on which the town of Claiborne stands—up to Perdue Hill, the upper strata of the White limestone, containing *Orbitoides Mantelli*, may be seen in many places near the road, thus proving the correctness

*The lower part of the Claiborne bluff below the ferruginous sand, which is spoken of by Tuomey, Winchell and others as a limestone, has no resemblance whatever to the "White limestone." It is in fact rather a series of fossiliferous calcareous sands, and calcareous clays, than a limestone.

I can further assert from personal knowledge, that there is, below what I have just defined as the White limestone, nothing whatever in the whole series of Alabama Tertiary strata (at least from the Alabama river westward), which could with any propriety be called a limestone, unless an occasional bed of indurated shell marl, seldom if ever exceeding 15 to 20 feet in thickness, might be so designated.

Meyer's section on p. 69 which he says must supersede all previously made sections, describes *d* as "color a bluish gray" 26 feet, but whether the material is sand, clay, or limestone, he does not say.

of Lyell's statement that at the bluff the upper part of the limestone had been cut off by erosion.

2. At Gosport, a few miles below Claiborne, there is an exposure very similar to that at Claiborne Bluff, and the White limestone appears on the hills a short distance from the river just as it was when Sir Charles Lyell observed it many years ago, and at a higher level than the Claiborne sands—as Lyell said it was.

3. At Rattlesnake Bluff, a few miles below Gosport, we have the Claiborne ferruginous sands with their overlying Scutella bed—about 18 feet above the water level. Above the Scutella bed occur some 15 feet or more of the White limestone.

4. At the mouth of Cedar Creek, several miles farther down the river, the Scutella bed is only about three or four feet above the water level with a few feet of the White limestone over it.

In Meyer's section of Claiborne—the ferruginous fossiliferous sands are 64 feet above the river level; at Rattlesnake Bluff they are 18 feet above, and at the mouth of Cedar Creek, just below the water level. These observations establish the fact of a southerly dip of the strata.

Mr. Meyer questions the fact of such dip, but there is no doubt about it, as may be seen below in 5. C. S. Hale, in this Journal, II, vol. vi, No. 18, Nov., 1848, in an article on the Geology of South Alabama, not quoted by Meyer, likewise demonstrates the fact of the southerly dip of these strata. He also distinctly says that the White limestone overlies the Claiborne ferruginous sands, and brings forward observations of his own to prove it.

5. If we go down the river from the mouth of Cedar Creek, during a low stage of water, we can have the direct evidence of superposition, of the relative places of the Claiborne sands and the White limestone, for the few feet of this latter rock above the Scutella bed, mentioned as forming the upper part of the bank at the mouth of Cedar Creek, can be followed down the river till they sink below the water level, but before one of these beds sinks out of sight another appears above it, and the succession of these low bluffs (hardly ever more than 10 to 15 feet high) is so nearly continuous, that practically all of the strata intervening between the Scutella bed at Cedar Creek, and the Orbitoidal limestone at Marshall's Landing, may be seen. These strata are mostly argillaceous limestones, glauconitic in places. At Marshall's Landing, this argillaceous limestone is distinctly overlaid by 10 feet or more of limestone filled with *Orbitoides Mantelli*. There is no trace of faulting or disturbance along this part of the river, and the uniform dip of the strata and their succession are unmistakable.

6. The most positive and irrefutable evidence of the relative position of the White limestone and the Claiborne ferruginous sands is to be seen at the St. Stephens Bluff. There can I imagine be no reasonable doubt that this bluff is made up of the White limestone. In its upper part this limestone is in places a mass of *Orbitoides Mantelli*, and in the lower part of the bluff about 15 feet above the water level is a hard projecting ledge, a foot thick, beneath which is a layer consisting of the shells of *Spondylus dumosus*.

A short distance—about half a mile—above the St. Stephens landing, this bluff is interrupted by a ravine, on the other side of which it may be seen again with identical characters; Orbitoides limestone forming the upper part as at the landing below. At the base of this part of the bluff occurs a red ferruginous sand full of fossils—among which are *Melongena alveata*, *Crepidula lirata*, *Infundibulum trochiformis*, *Corbula Murchisoni*, *Turritella lineata*, *Cytherea æquorea*, *Oliva Alabamensis*, *Turbinolia Maclurei*, *Voluta Defranckii*, *Astarte sulcata*, and a number of other forms which are found at Claiborne. Just over this bed occurs the Scutella bed above referred to, and over that, *in direct contact*, the white clayey limestone passing upward into the Orbitoidal limestone. This ought to set the matter at rest forever, so far as the relative positions of the Claiborne sands and the White limestone are concerned. I shall not here attempt to show that the White limestone of Alabama is the representative of the Jackson and Vicksburg strata of Mississippi; but I can assert from personal observations that the lower part of the White limestone of Alabama with its Zeuglodon bones and its limy soils corresponds with what Hilgard marks as Jackson on his map, and that the upper part of our White limestone is what Hilgard marks as Vicksburg. In p. 227 of Hilgard's Geology of Mississippi it is stated that the Orbitoides limestone is directly overlaid by the sandstone of the Grand Gulf group. This it seems to me is evidence that the Orbitoides limestone (which Hilgard considers Vicksburg) is at the top of the series with which we are concerned.

In paragraph 222, p. 144 of Hilgard's work, there is a distinct statement that the Vicksburg strata overlie those of the Jackson. The same thing is shown in his diagram No. 32.

b. From geographical position.

The Tertiary formations of Alabama have a southerly dip (S. or S.W.) which will average some 25 to 30 feet to the mile. This dip has been ascertained by L. C. Johnson and myself at several places, and that it is very nearly the average may be seen from the correspondence between our river sections and the records of a boring made at Bladon Springs. These data are very soon to be published.

I might also add in further proof of southerly dip, that all the streams flowing in easterly or westerly directions in the Tertiary territory of Alabama, have steep escarpments on the southern banks, and gentle southerly slopes on the northern.

That with a general dip toward the south, the Tertiary strata will outcrop across the country in approximately parallel belts, the newer beds appearing farthest south, is plain enough. Now in fact we have, going southward, the outcrops of the Tertiary formations in the following order:

1st, Lignitic; 2d, Buhrstone; 3d, Claiborne; and 4th, White limestone. There is but one exception to this order known to us, and that is of very limited extent, and plainly due to a flexure which can be traced out. In the same way, the Cretaceous outcrops lie to the northward of those of the Tertiary.

It seems to me that this relative position geographically of the outcrops of the formations, taken together with the general direction of the dip, would, to say the least, go far to prove their relative age.

Indeed the stratigraphical evidence of the superposition of the White limestone above the Claiborne sands is so abundant, so conclusive, and so completely without contradiction from a solitary fact, that it has, so far as I know, never been called in question by any geologist familiar with the field of their occurrence.

ART. XXXV.—*Native Antimony and its Associations at Prince William, York County, New Brunswick;** by GEORGE F. KUNZ.

THE Brunswick antimony mine is situated in Prince William Parish, York County, New Brunswick, on the right bank of the St. John River, 24 miles from Fredrickton, and 96 miles from St. John, New Brunswick. The locality was discovered about 1860, and a few years later the Prince William, the Hubbard and the Lake George mines† were opened; all of these were ultimately absorbed by the Brunswick mine. At the present time this company controls a tract of land three miles by five, containing many veins of stibnite, and forming one of the largest antimony deposits known.

In the early working of the mines, native antimony was only rarely observed, and was not even mentioned in the reports

* Read at the meeting of the Amer. Assoc. Adv. Science, August, 1884.

† This locality is briefly mentioned by Dr. L. W. Bailey in a letter to Professor B. Silliman, see this Journal, xxxv, 150, 1863. Messrs. Hayes and Jackson also mention this locality in the Geology of New Brunswick by Dr. L. W. Bailey, 1865. See also Report of the Geology of New Brunswick, 1865, by Henry Youle Hind.

of Professor G. I. Chase and Wm. Petherick, M. E., in their prospectus of the Brunswick Mining Co.* It is mentioned however in the fifth edition of J. D. Dana's System of Mineralogy, page 18, as of rare occurrence at the Prince William mine. The mining then was all merely on the surface, but within the last two years it has been carried on to a considerable depth. At a depth of from 100 to 150 feet the native antimony is occasionally found in large pockets, some of which contain fully one ton of the pure mineral; it is associated with stibnite, valentinite and kermesite.

In its most common form the mineral is very compact, and at times finely granular, in appearance, resembling very closely the native antimony of South Ham, Canada; in this form it breaks at times with a slightly conchoidal fracture and has a decided steel-blue color. The forms in which it occurs are usually either rounded or elongated masses, from ten to twelve inches across, and weighing from 20 to 50 lbs.; it passes into a coarse granular variety where it very closely resembles the mineral from Sarawak, Borneo. The most remarkable form, however, is that where it occurs in radiated masses of crystalline plates. The single blades are from one to two and occasionally four inches in length and $\frac{1}{8}$th of an inch across, being nearly as large as those of the artificial metal of commerce. The radiations seem to have massed around a common center, as if the mineral had cooled or crystallized slowly from without. One fine mass of this radiated tin-white metal measured over six inches across. Very small microscopic crystals about .05 mm. in size were developed by removing the calcite with acid, and mention was made of some large ones that had been observed, but these had been lost. Some of the crystalline cleavages have striations similar to those described by Laspeyres† that are also observed on the artificial antimony, and on the coarsely grained native variety from Allemont; these he refers to a polysynthetic twinning with $24R$ as the twinning-plane. The specific gravity of a pure piece of the coarsely crystalline bladed mineral was found to be 6·606 at 15°, and of the very compact, finely granular mineral 6·693. The following chemical results were obtained by Mr. James B. Mackintosh, E. M., School of Mines, New York City.

Coarse grained variety.

Gangue	0·84 per cent.
Iron	0·11 "
Arsenic	0·86 "

* Reports of Professor George I. Chase and Wm. Petherick, M. E., to the Brunswick Antimony Co., Boston, 1864.

† Zeitsch. deutsch. geol. Ges., 1875, p. 574.

Fine grained variety.

Gangue	5·04 per cent.
Iron	0·34 "
Arsenic	0·47 "

The difference in both cases is metallic antimony free from other admixture in large enough amount for detection, in the quantity used for the examination (1 gram.) In working the stibnite and accompanying ores on a large scale, to make the golden sulphuret (Sb_2S_5) gold and silver were observed in sufficient quantities to suggest the working of the ore for these minerals.

Stibnite occurs largely in the quartz veins in a massive form and in small diverging blades, and also in large masses with blades from four to six inches in length, and from one-quarter to one-half inch across. It occasionally occurs also in very small crystals in cavities, usually all of a very dark graphite color. In the seams between the layers of native antimony the valentinite occurs in massive and granular forms; also in beautiful radiations of bunched crystals, the radiations measuring over one and a half inches across. It occurs also at times in small hemispheres and in small isolated imperfect crystals not over 3 mm. in length.

Kermesite occurs in the cavities in stibnite and the antimony in small tufts of crystals, none of which were observed over one-half an inch in length; it occurs also in small hemispheres as a rule in the cavities in the native antimony. The color is from a dark cherry red to nearly black, and the fracture of the antimony is often streaked with this mineral. Dyscrasite, allemonite, senarmontite and native arsenic were looked for but not observed, although it is highly probable that some of these may yet be found.

The country rock is a black argillite, and the veins of quartz and calcite with stibnite which traverse it, vary from one to thirty feet; they are similar to the South Ham, Canada, deposits.* At the surface one of the principal veins had a dip of S. 80° E. with a depression of 55° N. Several other veins were N. 70° W. and S. 70° E. In following it from the points where the shafts were sunk in a westerly direction an increase of dip was observed.

I take pleasure in expressing my thanks to Mr. C. E. Parsons, of the Brunswick Antimony Co., for his kindness in furnishing information as well as specimens of the minerals for examination.

* Geology of Canada, 1863, p. 876. Report by Charles W. Willimott, Geol. Survey of Canada, 1880, 1881 and 1882, p. 3, G. G.

ART. XXXVI.—*The Crystalline Rocks of Alabama;* by C. H. HITCHCOCK, Hanover, N. H.

IN March and April, 1885, at the request of Professor E. A. Smith, State Geologist of Alabama, I studied the crystalline rocks of Eastern Alabama, and was thus enabled to compare them with the similar exposures in Virginia and New England. This region of exploration lies between the Coosa river and the east line of the State. It is the southern end of the long belt of crystalline rocks continuous from New Brunswick and seen farther east in Newfoundland. This area, as is well known, contains three groups of mountains, termed the northern, middle and southern sections. Viewed literally the western border is the most strongly marked, constituting the Green Mountains in New England, the Highlands in New York and New Jersey, and the Blue Ridge in Virginia. The more eastern portion rises to greater altitudes in the White Mountains of New Hampshire and the Black Mountains of North Carolina, while the depression between them is equally conspicuous and profound. The Alabama area lies wholly upon the southern slope of the last section, it being higher near the passage of the Georgia line by the Coosa river than about Auburn and Columbus. The country seems like an ancient table land worn to shallow depths by the tributary streams.

The geographical area to the west of this crystalline district is marked off definitely by that remarkable boundary, known as the Great Appalachian Lower Silurian Limestone valley, continuous from the Gulf of St. Lawrence to Eastern Tennessee and Northern Alabama. The contrasts on both sides of this boundary are well marked. On the east are the crystalline rocks, folded, inverted, faulted, traversed by igneous intrusions of the whole pre-Tertiary family of eruptives; on the west all the rocks are fragmental, posed in graceful folds or split by gigantic faults and are very rarely penetrated by unstratified rocks; on the east the rocks are principally Archæan; on the west they are Paleozoic; on the east the ranges are commonly short, with obtusely pointed summits; on the west the ranges are long barrows passing into plateaus. The eastern are the Alps and the western the Jura of America. It is this American Alpine section or the "Atlantic Primary Chain" of Featherstonhaugh which crowds the Coosa River in Alabama.

The geology of this region in Alabama may be best understood by a sketch of the order and position of the strata in a northwest-southeast section crossing them, say from Talladega to Columbus, Ga.

In Talladega the ubiquitous number II of Rogers—the

Lower Silurian limestone—is abundant. Near its eastern border are quarries of white marble not distinguishable in its appearance and general geological position from that of western Vermont. Not far removed from it is the familiar quartzite with its *Scolithus*—the same rock whether in Vermont, Pennsylvania, Virginia or Alabama, and it everywhere forms mountain ranges, as about Talladega and Anniston. As in Vermont, so in Alabama it requires close study to discover the true relations between this sandstone, the marble and the various limestones. The calcareous portion attains a greater thickness than in the north and it has been well divided by Professor Safford in the various members of the Knox group, which have not yet been identified *seriatim* (to my knowledge) north of Virginia.

The range of mountains immediately adjoining these limestones has the Ocoee group of Safford upon its western flank and summit, and it attains the highest elevation of any of the summits, and is to be compared with the foot hills of the Blue Ridge which sometimes surpass the main ridge in magnitude. The rocks are greenish and drab slates and schists, argillaceous, nacreous and hydromicaceous, together with layers of quartzite which in some localities attain great thicknesses. Grains of quartz presumably water-worn, are disseminated through many of the schists. All the strata dip southeasterly, more at first than afterwards, so as to give the aspect of an overturned anticlinal. Small veins of quartz, cavernous through decomposition and with bunches of chlorite, further characterize this group. I find from Safford's descriptions that the rocks of the typical area of this formation in Tennessee are similar. I have not seen much of this member in Virginia. Whatever of it is to be found there was placed by W. B. Rogers in his formation No. 1. But the exact analogue of this group is to be found in the magnesian slate of Emmons in western New England and eastern New York, whether it be Lower Silurian, Acadian or Huronian; a hundred typical specimens collected in Alabama could not be distinguished from a corresponding number obtained in the Taconic Mountains of western New England. This character is uniform in the three places where I observed it, near Anniston, Talladega and Syllacauga.

The next band of rock, usually just to the southeast of the crest of the mountains, is a feldspathic mica schist, or an imperfect gneiss. It resembles the inferior grades of the Green Mountain gneiss of Vermont. Near Ashland a few ledges remind one of the schists of Mt. Washington, N. H., but no more so than does the Green Mountain rock. There is not enough of this rock to enable us to call it Montalban.

Two bands of a newer schist are intercalated within this

mica schist. The first is developed at Chandler's Springs as a heavy greenish chloritic schist: the second may be seen near Millersville or Hillabe and is a garnetiferous argillaceous schist: the first is like the familiar green rock east of the Green Mountains in Canada, Vermont and Massachusetts, holding beds of steatite and serpentine—the second is a genuine synclinal. Passing farther along we find next a broad belt of well defined gneiss between Pinckneyville and Alexander City. Part of it is reticulated by the segregated veins which are characteristic of the Lake Winnipiscogee gneiss of New Hampshire. Another portion is a genuine granite of oval shape about six miles in length. It is much like the Amoskeag quarry granite of Manchester, N. H., and its presence is marked by immense bowlders of decomposition, a novel sight to northern geologists. A few miles beyond Alexander City, at the crossing of the Tallapoosa River the rock is full of pyroxene, and farther along it is associated with gneiss standing vertically.

Midway between Alexander and Dadeville is a breadth of one or two miles of the augen gneiss. Between this and Auburn, gneiss of diversified aspect and considerable hornblende schist prevail. In the vicinity is a good locality of the mineral corundum. At Ragen's mill on Songahatchie creek near Loachapoka a massive gneiss is quarried for underpinning and piers, and is cut by a dike of gabbro similar to that in the White Mountains.

Southeast from Auburn are repetitions of the augen gneiss. Beyond them are the Chewackla quarries of limestone which are associated with itacolumite. These are very like the quartzites and limestone of Thomaston, Rockland and Camden in Maine, and probably those of Smithfield, R. I. A large business is done in the manufacture of quicklime at Chewackla and other localities near by. These northern rocks were referred to the Taconic system by Emmons. All of them, including similar deposits in the Carolinas near the eastern border of the crystallines, evidently originated in the same geological period, whether that be Cambrian or Silurian.

The remaining thirty miles of the section, from Opelika to Columbus were traversed along a railroad. Gneisses predominate with variable dips. Midway and toward the southeastern end the heavy massive gneiss near Auburn is repeated. At Columbus, the exposures are closely similar to the saccharoidal gneisses of Manchester, N. H., and to the thoroughly crystalline schists of typical Laurentian areas. Between Alexander City and Columbus, any geologist familiar with the Canadian or Adirondack gneisses will constantly recognize the peculiar features of that ancient fundamental gneiss. Our examinations terminated at Columbus, as we here reach the Atlantic Tertiary

plain, at the head of navigation for large steamboats. A return trip northward near the State line enabled us to correct and confirm the impressions derived from the study of the section.

Certain obvious conclusions may be briefly summarized. First as to structure: (*a*) The contact between the Silurian limestones and the Ocoee group is not the natural one. There is no deep cut like that across the Blue Ridge at the James River to show the two groups in their undisturbed primal position. (*b*) The Ocoee slates dips are small, and when protracted upon a scale there seem to be an inverted anticlinal in the Syllacauga section. This position is due to the tangential motion of the elevating force and there is no reason to doubt Safford's assignment of these slates to the horizon suggested by the geographical position, — between the Potsdam and the ancient gneiss. (*c*) The first mica schist or gneissic band, as in so many sections farther north is an inverted anticlinal with many subordinate folds. As a careful study of this group at the north develops unexpected normal anticlinals, so it will probably be the case in the south. (*d*) Belts of chloritic and argillaceous schists, when conforming to the adjacent monoclinal gneisses or coarsely crystalline mica schists, are supposed to indicate compressed folded synclinals. Hence besides the western anticlinal between the Ocoee range and Ashland there must be at least one inverted synclinal of the chloritic schists if not others in the older group. (*e*) The second or argillaceous belt is synclinal in attitude and so are the underlying gneisses, between Ashland and Alexander City. It may be convenient to term the chloritic belt the Lower and the argillaceous belt the Upper Huronian. (*f*) Dadeville, in the midst of the indisputable Laurentians, is the center of a very large synclinal.

Second, as to mineral resemblances: (*a*) No one will hesitate to accept the identity of the limestones and quartzite in the edge of the great Appalachian valley with the corresponding rocks in Virginia and New England known as the Lower Silurian and Potsdam sandstone. (*b*) The Ocoee corresponds best with the later Huronian or the earlier Cambrian of the north. There is no good reason for calling it Acadian. (*c*) A section across northern New England has many points of resemblance to the one in Alabama described above. In northern Vermont after leaving the Potsdam there is a band of argillitic schists very like the Ocoee. This lies west of the Green Mountains and by Zadock Thompson in an unpublished map was once called the Magnesian slate of the Taconic system. The Green Mountain gneissic anticlinal follows with a repetition of the Ocoee-like slates and also the heavy chloritic group with serpentines referred to the Huronian by most authors.

Within this Huronian synclinal, between the Green Mountains and Connecticut River, lies a micaceous, argillaceous often calcareous group, which is the latest formation in that section of country. The east side of the Huronian synclinal is underlaid by better characterized gneisses than those upon the west side—and on reaching the watershed between the Connecticut and Merrimack Rivers in New Hampshire we find the augen-gneiss, which is regarded as the oldest rock in the north. Between this ridge and the ocean there are several synclinals filled with slates, quartzites or limestones of various descriptions. The order in Alabama is the same, save that there is a peculiar development of mica schist at several localities, particularly in the White Mountains, and called Montalban in my writings, which is not yet recognized. (*d*) Throughout the Atlantic crystalline area the oldest of the gneisses lies from thirty to fifty miles east of the Green Mountains and Blue Ridge range. (*e*) Having had the opportunity to examine a copper vein in Alabama, said to belong to the Ducktown type, I recognize the familiar mineral character of the copper regions of Ely, Strafford and Corinth in Vermont. The locality is the Woods Mine, Cleburne County, fully described in Professor Smith's report of progress for 1874. These veins occur quite near the junction of hornblendic schists with mica schists. The iron pyrites is the pyrrhotite. These veins are very different from those of Lyman and Milan, N. H., and those near Lennoxville, P. Q., which lack the pyrrhotite and are imbedded in the Huronian. The Ducktown and Vermont copper regions were of later origin than the others, and might be termed later Huronian, corresponding to the Montalban of Dr. T. Sterry Hunt.

Final scheme of classification.—Perhaps for the present it may not be needful to depart from the distinction laid down by Logan in 1855 for the crystalline series, that of Laurentian and Huronian. The beds commonly recognized as gneiss belong to the former, while the chloritic, hydromicaceous and argillaceous schists are chiefly Huronian. The latter are largely of sediment derived from the pre-existing gneisses and granites.

It has been necessary to speak of different sections of these two great groups, and the terms upper and lower have been employed. No term of geographical origin need be employed to define the places of the several divisions. The Laurentian of the east may be subdivided into lower, middle and upper or the augen gneiss at the base, followed by well characterized heavily bedded gneisses for the middle division, and coarse mica schists or imperfect gneisses for the upper part. The Lower Huronian would be the massive chloritic division, while the upper part is more varied, being argillaceous, hydromicaceous, nacreous and quartzose.

We may regard the primary division into Laurentian and Huronian as thoroughly established, and hence are warranted in using lithological distinctions to guide us in discriminating the age of crystalline rocks so far as they can be derived from the predominant features of these great systems. For a long time to come their further subdivisions will be open to question.

ART. XXXVII.—*The Geometrical Form of Volcanic Cones and the Elastic Limit of Lava;* by GEORGE F. BECKER.

General character of a volcanic cone.—If a fluid or partially fluid substance, such as lava, issues from an orifice in a plain and congeals about the mouth, the accumulation will form an elevated mass more or less nearly resembling a cone. If the resistance of this solid at the base is greater than the load, matter will or may continue to accumulate on the upper portion, while if the resistance towards the base is smaller than the load, a readjustment of material will necessarily take place. It is to be inferred, therefore, that under favorable circumstances volcanic cones will approximate to some definite form, and this inference is strengthened by the well known fact that the form of craters has been closely imitated by experiments on plastic material. Observation, too, shows that volcanic cones are characterized by a remarkable regularity of outline and that the outlines of the more regular cones of volcanoes wherever observed are strikingly similar.

Milne's results.—Professor John Milne has discussed the form of volcanic cones,* but his papers have not received the attention they deserve, containing, so far as I know, the first attempt to assign a definite geometrical form to a natural surface.† Though his papers are cited in some recent works on geology, I have seen no reference to this fact and it had entirely escaped my own attention until the material for a paper on the subject was completed. My results are closely

* Geol. Mag., vol. v, 1878, p. 338; and vol. vi, 1879, p. 506.

† Geology is one of the least exact of sciences, and opinions still differ among the ablest specialists as to its most fundamental doctrines; but, so far as geological relations are exactly known, they are necessarily capable of mathematical expression, and all relations are essentially exact, however imperfectly we may be acquainted with them. Per contra, the investigations of Sir William Thomson, Professor G. H. Darwin and other physicists show what grand additions may be made to geological science by the application of mathematical reasoning, and it appears to me that geologists should be on the watch for every opportunity to achieve for any geological relation, however insignificant it may appear, that final intellectual conquest which is symbolized by its correct expression in mathematical language. As in other branches of theoretical physics blunders will be made and progress will be slow, but opinion will gradually yield to certainty over a portion of the infinite field.

analogous to Professor Milne's but differ in some important particulars, and the data accumulated by him enable me to carry the subject a little further. Professor Milne regards the problem of a volcanic cone as that of the form which would be assumed by a mass of loose ash, and he is led to the conclusion that the form which would be assumed by such material would be that generated by the revolution of a logarithmic curve round its asymptote.* His experiments on sand, however, as he points out, gave him only right cones with straight sides, excepting when material of different sizes was employed and the action was such as to involve a sorting of the particles. I cannot think that the form generated by the revolution of a logarithmic curve about its asymptote is the figure of stability for a mass of loose material. Without any analysis it seems beyond question that were a long trumpet-shaped tube filled with dry sand, inverted on a horizontal plane and the tube withdrawn, the column of sand would collapse, and that a cone or some figure very similar to it would result. On the other hand it is a well known fact that the solid produced by the revolution of the logarithmic curve about its asymptote is the form which a loaded column of uniform strength must have when the material of the column is continuous and coherent like metal or stone. Such a column may be cut at any point and the load it will bear is the weight which the infinite column above this section would possess were the material uniform.† This, however, is not exactly the problem of the volcanic cone, which is an unloaded finite column, and neither an infinite one nor, what amounts to the same thing, a finite one supporting an extraneous weight.

Solid unloaded column of "least variable resistance."—A part of the ejecta of a volcano is melted lava and congeals to rock. A large part is also in the form of sand or "volcanic ash," but of this much is indurated to a tolerably firm material after ejection by one cause or another and, although weathering and frost will often cover the slopes of a mountain such as Fusi-yama with detritus, it appears to me that volcanic cones must be regarded as essentially continuous masses.

Suppose a columnar mass of uniform coherent material, the surface of which is generated by the revolution of some curve about a vertical axis. Let the height of this column be a, its radius y, the distance of any horizontal plane from the base x, the specific gravity of the material ρ, and the coefficient of resistance to crushing stress at the elastic limit κ. If such a

* Professor Milne does not give his conclusion in this form, but states that the sum of equally spaced ordinates will be to their difference in a constant ratio. which is equivalent to the statement in the text.

† The volume of this solid of revolution is of course finite in spite of the fact that its length is infinite.

column is in a condition of stable internal equilibrium, the total resistance of any section to crushing stress must at least equal the pressure of the load resting upon the same section, so that if p is the pressure of a load which would strain a given section to its elastic limit, or the possible load, and if l is the actual load, $p \geqq l$ and $\frac{dp}{dl} \geqq 1$. The square of this differential coefficient, if not unity, must also exceed it, so that

$$\frac{dp^2}{dl^2} = 1 + \mathrm{F},$$

where F denotes some positive quantity the limiting value of which is zero. It follows from the definitions that

$$p = \pi\kappa y^2; \qquad l = \pi\rho \int_x^a y^2 dx;$$

and therefore

$$\frac{dp^2}{dl^2} = \left(\frac{2\kappa}{\rho}\right)^2 \frac{y'^2}{y^2}$$

the accent as usual denoting the differential coefficient taken with regard to x. The volume of the solid of revolution above the section at x is therefore

$$\pi \int_x^a y^2 dx = \pi \int_x^a \left(\frac{2\kappa}{\rho}\right)^2 y'^2 dx - \pi \int_x^a \mathrm{F} y^2 dx.$$

For the limiting value F=0, or when the load everywhere exactly equals the carrying power, these equations give for the generating curve

$$y = \mathrm{A}\, \varepsilon^{-x\rho/2\kappa},$$

a well known formula for this condition. Since this logarithmic curve meets the axis only at infinity, the condition $l=p$ answers only to an infinite value of a.

In the equation for the volume, either term may be regarded as varying independently. If the term containing y' is supposed constant, the volume above any and every section will be a maximum, for stated values of the constants, when the term containing F is a minimum; and this must be true even when a is not given the special value ∞. Consequently when this term is a minimum, each section of the column will be loaded as nearly to the elastic limit as the conditions will permit and the entire column will contain less material than any other stable column of the same height, or, for a given amount of material, will be the highest stable column which can be generated by the revolution of a continuous curve, or which can be formed under the action of a uniform law. This postulate of continuity of course excludes from the possible finite

forms the truncated logarithmic column, with or without an imposed load. It appears to me that a volcanic cone, formed as indicated at the beginning of this paper must fall under this problem.

For the sake of brevity, I propose to call the solution of this problem the form of *least variable resistance*, a term intended to include as a special case the form of uniform resistance or the infinite logarithmic column. The form of least variable resistance then is a solution of the equation

$$\int_z^a \left(y^2 - \left(\frac{2\varkappa}{\rho}\right)^2 y'^2\right) dx = \text{min.},$$

or by the method of variations

$$\delta \int_z^a \left(y^2 - \left(\frac{2\varkappa}{\rho}\right)^2 y'^2\right) dx = 0,$$

giving

$$y = \left(\frac{2\varkappa}{\rho}\right)^2 y'',$$

or

$$y = A\varepsilon^{-x\rho/2\varkappa} + B\varepsilon^{x\rho/2\varkappa},$$

where A and B are arbitrary constants.

If the origin of the curve is now taken at the center of the base of the solid of revolution, and if for convenience $\frac{2\varkappa}{\rho}$ is made equal to c, y must disappear for $x=a$ and therefore

$$B = -A\varepsilon^{-2a/c};$$

$$y = A(\varepsilon^{-x/c} - \varepsilon^{-2a/c}\varepsilon^{x/c}).$$

When $a=\infty$, $\varepsilon^{-2a/c}=0$ and the equation reduces to the simple, well-known logarithmic form of uniform resistance. If the origin is taken at the summit of the cone, and both x and y are multiplied by c, the equation may be written

$$y = b(\varepsilon^{-x} - \varepsilon^{x}).$$

Here b must be determined to correspond to the equation of condition which becomes

$$\int_{-z}^{0} (y^2 - y'^2)\, dx = 4b^2 x = \text{min.}$$

The more b exceeds b^2 therefore, the smaller will be $4b^2x$. Now it is well known property of $\frac{1}{2}$ that it exceeds its square more than any other number, and consequently $b=\frac{1}{2}$. Introducing this value and restoring c the equation becomes

$$\frac{y}{c} = \frac{\varepsilon^{-x/c} - \varepsilon^{x/c}}{2}$$

and this is the equation of the curve which by its revolution about the x-axis will generate the finite unloaded column of "least variable resistance."*

This is the same curve barring the value of constants which I have shown twice elsewhere,† characterizes step faults. It might have been deduced directly from the discussion presented in the latter paper on the distribution of energy in compressible masses under the action of a constant force, or that discussion might have been evolved from this. The arrangement of sheets of rock in a complex fault, the distribution of pressure in the atmospheric column, the form assumed by a cold rivet, and the shape of a volcanic cone, as well as some other important cases, and possibly some vegetable forms‡ are mere variations of a single problem and find their solution in different readings of the equation

$$w = A\varepsilon^{-x/c} + B\varepsilon^{x/c}$$

where w is the energy potentialized per unit volume.

The quantity $\frac{2n}{\rho}$ is the natural unit of the volcanic curve corresponding to the constant sub-tangent of the infinite form and to the constant of the catenary curve. It is of course constant for any given homogeneous material and different for different materials. Consequently solids of different materials

* Whether this proposition is new or not my reading is insufficient to determine. I can only say that I have looked for it in vain in a number of treatises in which it might have been expected to be mentioned if known.

† Geology of the Comstock Lode, chap. iv; Impact Friction and Faulting, this Journal, vol. xxx, p. 116.

‡ The case of a loaded column of uniform strength seems unlikely to be met with in inorganic nature for it appears to imply an adjustment of the column after the imposition of the load. I strongly suspect however that the simple logarithmic column is the form to which tree trunks tend in forests where the influence of winds is but little felt. Where such trees reach a large size and especially where the wood is soft, the increase in diameter near the ground is very marked. Thus in the red-wood forests of California the largest trees are generally cut some ten or more feet above the ground to save the inconvenience of handling a trumpet shaped log. This increase of diameter is less marked in trees of moderate size than in very large ones and less among hard wood trees than in species the wood of which is soft. Forest trees of course seek the light, and one can scarcely doubt that they reach it as rapidly as it is possible to do so consistently with stability. If so the load at any section below the branches per unit of area of this section will be a maximum and will be the same at all sections, and if this is true the form is the simple logarithmic column. For if F is the area of the section or πy^2, and if F_0 is the value of F for the datum plane, the equation may also be written

$$F = F_0\varepsilon^{-x\rho/\kappa}.$$

This would lead to a simple means of testing the question under favorable circumstances. If one were to cut a well developed forest tree just below the branches and divide the trunk into two or more portions, weigh the branches and each log, and measure each cross-section, it could of course be determined in a moment whether the load per square inch of all sections were uniform or not.

corresponding to this equation will differ only in scale or will be geometrically similar. The value of $\frac{2\varkappa}{\rho}$ can easily be expressed in terms of x and y. It will be found that

$$\frac{4\varkappa^2}{\rho^2}y'^2 - y^2 = \frac{4\varkappa^2}{\rho^2}$$

which gives

$$\frac{2\varkappa}{\rho} = \frac{y}{(\tan^2\vartheta - 1)^{\frac{1}{2}}}$$

ϑ being the angle which the tangent at any point makes with the x axis.*

In the figure of least variable resistance the radius becomes zero when $\vartheta = 45°$. Below the point of the cone ϑ increases with the radius. In comparing the theoretical form with actual occurrences this angle is especially significant.

Comparison with actual cases.—Whether or not the figure of least variable resistance is that of a volcanic cone, can, I take it to be, determined only by comparison in spite of the apparently good reasons which have been stated for such a supposition. The first step for such a comparison is the reduction of each drawing or of corresponding numerical data to the same unit, which can be done by help of the formula for $\frac{2\varkappa}{\rho}$ just given. Professor Milne gives diagrams and tabulated measurements of Fusiyama and Kumagatake which I have attempted to reduce in this way. The diagrams were taken from selected photographs and are probably slightly but certainly not greatly distorted. Their actual scale is unfortunately not given. For the left side of Fusiyama Professor Milne gives in centimeters the position of eleven points on the section referred to the axis of the volcano. Of these I rejected the two uppermost for reasons to be mentioned presently, and calculated $\frac{2\varkappa}{\rho}$ for each of the others, assuming that the cord connecting any two points was parallel to the tangent at a point half way between them. By reference to Professor Milne's diagram it will be seen that his points are so close that

* For the infinite logarithmic column on the other hand

$$\frac{2\kappa}{\rho} = \frac{y}{\tan\vartheta}$$

while for the catenary

$$y = \frac{c}{2}\left(\varepsilon^{-x/c} + \varepsilon^{x/c}\right)$$

$$c = \frac{y}{(\tan^2\vartheta + 1)^{\frac{1}{2}}} = y\cos\vartheta.$$

no sensible error is involved in this assumption. The average value of $\frac{2\varkappa}{\rho}$ obtained was 2·86 centimeters. To reduce the diagram to any desired unit, c, it is only necessary to redraw it to a scale in which $\frac{c}{2\cdot86}$ is treated as unity. The value of c which I happened to find convenient was 4^{cm}. I drew both sides of Fusiyama to the same scale to which I had plotted the equation of the solid of maximum stability and give the plots below. I treated Kumagatake in the same way, getting the value of the natural unit in Professor Milne's diagram as $2\cdot61^{cm}$.

In fig. 1 may be seen the theoretical locus and Professor Milne's outlines of these cones reduced as described. They are drawn to the same axial line but to different bases, so that for purposes of exact comparison a tracing of one should be made and shifted vertically until it more or less nearly coincides with the other. If this is done, a similarity will be revealed between the results of theory and the facts which seems to admit of but one explanation.

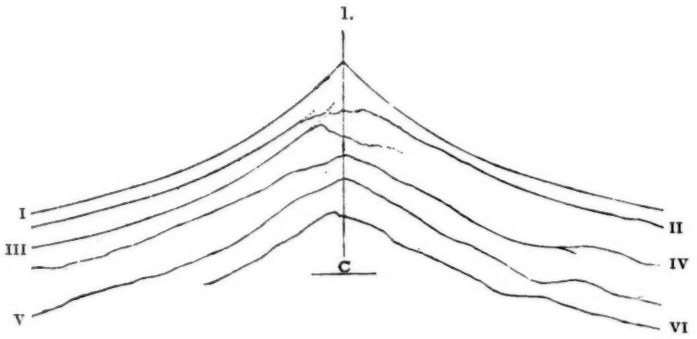


FIGURE 1.—Theoretical curve and outlines of actual volcanoes. The comparison is to be made by vertical transposition. I, is $y=\frac{\varepsilon^{-x}-\varepsilon^{x}}{2}$; II, Milne's outline of Fusiyama; III, Milne's outline of Kumagatake; IV, Shasta, enlarged from Taber's stereoscopic view from north side, No. 1542; V, Hood from the Dalles, enlarged from Watkins' New Boudoir Series, D. 61; VI, Popocatapet'l enlarged from A. Briguet's photos of Aqueduct of Tomacoco Mill, No. 19; c, the unit or $\frac{2\kappa}{\rho}$.

I may mention that the slopes as given by Professor Milne are reproduced as accurately as it is practicable to draw them to an altered scale, and that the natural units obtained as stated were used without any correction or adjustment. That for Fusiyama might be changed a trifle, for, according to my computations, Professor Milne has not taken the axis quite at

the center. On recomputing it however for the slight discrepancy mentioned, I find that the difference in the cross section of the mountain would be imperceptible on the scale of the figure, and I therefore prefer to present it exactly as it results from Professor Milne's measurements. A small part of the lower portions of the left sides of the mountains as given by Professor Milne are omitted because they could not be included without unduly reducing the scale of the cut. These portions correspond as well as the remainder with the theoretical form. Professor Milne gives no more of the right side of Kumagatake than the cut shows.

In computing the value of the natural unit I rejected the two measurements nearest the peak because the summit is not only most subject to erosion when not snow-capped but should not theoretically coincide with the figure of least variable resistance; for such a coincidence would imply an infinitesimal crater while the existence of an actual crater implies the presence of less matter, or a smaller load, close to the summit and consequently a more rapid convergence of the sides. Strictly speaking, the presence of a crater would affect the whole figure, but the influence of this diminution of the load will manifestly be extremely slight excepting near the crater itself whenever the crater is small compared with the volume of the cone.

For the sake of comparison with these very perfect examples I have introduced outlines of Mt. Shasta, Mt. Hood and Popocatapet'l carefully reduced to appropriate scales from photographs. These mountains are all rather irregular but will serve at least to show a striking similarity in the curvature of volcanic cones, and a pretty close agreement with the theoretical form. This likeness can best be judged of by making a tracing of the theoretical cone and placing it upon the outlines of the mountains. Professor George Davidson has kindly lent me a sketch of Mt. Renier, which he made for the special purpose of recording its slopes. Long practice in this kind of work makes him confident that this sketch is correct as to angles to something like one degree. The sketch coincides most remarkably with the theoretical form but is not added to the diagram because its evidence is scarcely comparable with that obtained mechanically by photography. In the case of a very large and deep crater it might be interesting to compare the form of greatest stability with that of the wall of the crater. If R were the radius of the outer surface of this wall at any level and r the radius of the inner surface the area of the ring would be $\pi(R^2-r^2)$ and $(R^2-r^2)^{\frac{1}{2}}$ would have the same value as y in the equation given for a pointed cone.

Professor Milne states that the highest slope he has observed

upon a volcano is 40° upon Kumagatake, while he notes in pictorial representations of volcanic cones angles often exceeding 50° and one reaching 69°. He is inclined to think that artistic feeling may have induced exaggerations in these cases. Other geologists have of course also called attention to such misrepresentations. If my theory of the form of volcanic cones is correct these objections are well founded, since 45° is reached only at the (impossible) point of a solid cone. If the thin walls of large craters, however, are sufficiently solid to take a form of least variable resistance higher angles than 45° will occur.

The elastic limit of the average lava of cones.—Besides its geometrical importance in the equation of the volcanic cone the quantity $\frac{2\varkappa}{\rho}$ possesses a further property of at least equal interest. The coefficient of resistance at the elastic limit of the material of the cone is

$$\varkappa = \frac{\rho}{2} c.$$

Now $\varkappa$ is a constant which it is peculiarly difficult to determine experimentally for any material, while it is one of prime importance in the grand question of geology, upheaval and subsidence. The value of c, however, can be immediately derived from observations on volcanic cones or from drawings to scale or from photographs of which the scale and the angle of the plane of projection to the vertical are known, while ρ is determinable for uniform material with the utmost accuracy and ease and a close approximation to its average value could doubtless be obtained by a considerable number of experiments for the materials of almost any volcanic cone. The value of ρ is capable of being further checked by the results of pendulum observations. The form of the more regular class of volcanic cones will therefore enable geologists to determine the modulus of resistance for the elastic limit on an enormous scale for an extremely important class of the constituents of the earth's "crust," and if the scale should prove not to compensate for the uncertainty as to the value of the density, the method cannot fail to afford a valuable check on those obtained from laboratory experiments.

Mr. Mark B. Kerr of the U. S. Geological Survey has kindly furnished me with a surveyed section of the "Sugar Loaf," Siskiyou County, California. It is shown in fig. 2 with the theoretical curve on a larger scale than that employed in fig. 1. The agreement is very good and the scale being known gives

$$c = \frac{2\varkappa}{\rho} = 2560 \text{ feet} = 780{\cdot}26 \text{ meters}.$$

The height of a uniform column of this material which will strain its lower surface to the elastic limit is

$$\frac{\varkappa}{\rho} = 1280 \text{ feet} = 390 \text{ meters.}$$

If its specific gravity is 3, the load it will bear at the limit of elasticity per square centimeter is 117 kilos. This is a very reasonable result, for I find in a table compiled from a number of trustworthy sources the following values for the pressure at the breaking point (which is of course greater than that at the elastic limit). Good brick, 100 kilos. pr. sq. cm.; sandstone, 200; limestone, 300; granite, 600. It is probable that the mean specific gravity of the cone is below 3 and its carrying power not much above that of good brick. A good suite of specimens would be necessary to give this determination much value because of the uncertainty as to the density of the material, but it at least exhibits the method.*

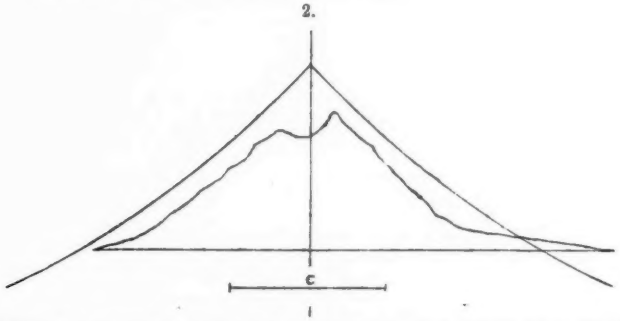


FIGURE 2.—Theoretical curve and surveyed section of Sugar Loaf, Siskiyou County, California. The comparison is to be made by vertical transposition. Elevation of base, 4000 feet; of summit, 6399 feet. $c = \frac{2\varkappa}{\rho}$.

Lunar volcanoes.—If ρ is given the form mg, where m is the mass of the unit volume and g the acceleration of gravitation, c or $\frac{2\varkappa}{mg}$ is inversely proportional to g. Hence, if the attraction of gravitation at the earth's surface were to diminish c would

* Since this paper was completed I have had access to the maps of Mt. Shasta, recently made by the U. S. Geological Survey. For the sake of testing the theory set forth above four vertical sections through the summit were prepared by the topographical assistants on lines running north and south, east and west, northwest and southeast, northeast and southwest. All of these profiles showed a very satisfactory general agreement with the theoretical form and yield a value for κ/ρ of about 1320 feet or within 40 feet of that found for Sugar Loaf, so that considering the roughness of the method the two results are to be regarded as substantially identical. The main mass of Shasta appears to consist of andesites, somewhat "trachytic" in texture.

increase and, *ceteris paribus*, loftier volcanic cones would result. If, therefore, the material of the volcanic cones on the moon closely resembles that of those on the earth, the enormous height of lunar volcanoes is in part ascribable to the feebleness of the attraction which the moon exerts upon bodies at its surface. On the other hand, studies of the form and dimensions of lunar volcanoes would lead to values of $\frac{\varkappa}{\rho}$, from which it might be determined approximately whether the height of a column of lunar lava which would strain its lower surface to the elastic limit does or does not correspond to that of columns of terrestrial lavas; a step of some interest in lunar physical geology, and one which might even lead to a guess as to the lithological character of the lunar rock, since different lavas probably have different characteristic values of $\frac{\varkappa}{\rho}$.

Office U. S. Geol. Survey, San Francisco, Feb., 1885.

ART. XXXVIII.—*Notice of a new genus of Pteropods from the Saint John Group (Cambrian)*; by GEO. F. MATTHEW.

IN studying the organic remains of the St. John Group the writer has met with a new genus of Pteropods which is of interest as showing the relation of the ancient genus *Hyolithes* and its allies to the Cephalopods.

Eichwald's genus *Hyolithes* was based on a species which is camerated near the apex; but the new genus not only has this chambered area near the apex, but is also divided by diaphragms on one side of the shell, nearly to the aperture, somewhat in the manner of *Phragmotheca* of Barrande. It may be described in the following terms:—

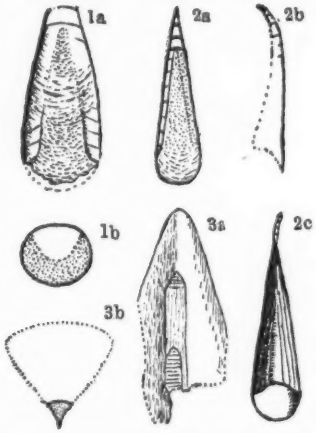
DIPLOTHECA n. gen.

Slender oval cones somewhat triangular in section with abbreviated or attenuated apices. In the narrower part of the tube or cone there are several septa that divide off segments of the tube from the body cavity (chamber of habitation). The body cavity is separated from one side of the outer shell by a thin partition supported by delicate transverse septa or diaphragms. The apex in one species is prolonged into a narrow attenuated flexible tubule with transverse annulations (diaphragms ?) at regular intervals.

This genus differs from *Camerotheca** (another group of *Hyalithoid* shells associated with it in the same measures) in the more rapid enlargement of the shell during growth (which

* Described in the Canadian Record (Canadian Naturalist), 1885.

thus formed a shorter cone than *Camerotheca*), and in having a firmer and, as preserved in the shales, rounder side, where it has the support of the lateral diaphragms. This feature of the endo-skeleton is most distinct in two species, varieties of which are found in the sandstones near the base of Div. 1 of the St.



John Group. The shells of these pteropods are here found scattered over the surface of the sandy layers of an old sea bottom, close to the shore line; and occur mingled with the material forming the casts of worm burrows and imbedded in phosphatic nodules. They thus occupied locations where in later formations lamellibranchs might be looked for: but the remains of numerous individuals of the species of this genus are also found in fine shale at a higher horizon, showing that the genus inhabited deeper waters as well.

Further particulars of these species will be found in an article read before the Royal Society of Canada, 1885.

Reference to the figures:

1a. *Diplotheca acadica* Hartt, sp. var. *crassa*, dorsal view, showing the diaphragms of the endoskeleton. The diaphragms near the aperture and the septa near the apex are exposed by the abrasion of the shell.

1b. Same, transverse section, showing the body cavity partly enveloped by the phragmated part of the tube (represented by the shaded area).

2a. *Diplotheca Hyattiana*, $\frac{4}{1}$, vertical section, showing the septa at the apex of the tube, and the diaphragms at the side.

2b. Same, section of a flattened shell, from front to back, showing the curved apex.

2c. *Diplotheca Hyattiana* var. *caudata*, $\frac{2}{1}$, showing the annulated flexible tubule attached to the apex.

3a. *Phragmotheca Bohemica* Barr., showing along the axial line the place where the phragmated sheath is found and the closeness of its diaphragms—figured for comparison.

3b. Same, transverse section of the shell, the shaded portion marks the narrow phragmated sheath.

St. John, N. B., July, 1885.

ART. XXXIX.—*Cope's Tertiary Vertebrata;** by J. L. WORTMAN.

THE exceptional facilities which this country affords for researches into the history of its extinct vertebrate inhabitants have been well known ever since the rich fossiliferous deposits of the Rocky Mountain region were first brought to the attention of students of geology.

The unusual qualifications necessary for a successful prosecution of investigations in this branch, as well as a general lack of the requisite facilities in the way of large osteological collections in this country, have no doubt prevented many from entering this field of study.

The expense likewise attendant upon the collection, preparation, and illustration of material is so considerable that only those who have a comparatively large amount of means at their disposal can make much headway in it. While these causes have necessarily limited the number of investigators to an extent by no means commensurate with the material to be investigated, activity has nevertheless been proportionately great since Leidy, Cope and Marsh began making collections of fossil remains in the West. Later Scott and Osborne of Princeton have been more or less actively engaged in the same pursuit.

As a result of the study of these collections great contributions to our knowledge of the extinct vertebrate fauna have been made. Those of Leidy were the first, and their superior excellence must always remain a monument to his scholarly attainments.

Contributions by Cope and Marsh have followed from time to time, some of which have been complete and handsomely illustrated; their value is of high order. Up to within a comparatively short time however, the greater part of their immense collections, especially those from the Tertiary horizons, have been made known only through preliminary descriptions and stray papers in which little else was attempted than a brief and hasty description of the most salient characters of the new species and genera discovered.

Within the present year two notable quarto volumes have been issued by the U. S. Geological Survey, forming the most considerable contributions to the subject which have yet appeared in this country. One of these is by Professor E. D. Cope of Philadelphia, and is devoted to the vertebrate fauna of the older Tertiary deposits of Western America. It comprises somewhat over one thousand pages of text, which is illustrated

* U. S. Geolog. Surv. Territories. The Vertebrata of the Tertiary formation of the West. Book I. By Edw. D. Cope. Washington, 1883–1884.

by one hundred and thirty-three beautiful lithographic plates. As it covers but little more than one-half of the entire Tertiary fauna, it is soon to be followed by another volume of equal dimensions which will be devoted to the later horizons.

To students of Mammalogy, this work will prove of unusual interest on account of the flood of light which it throws upon the origin and relationship of many groups which have hitherto proven puzzles to the best zoologists. One of the leading questions in the study of the Mammalia is their origin and succession. It is well known that they make their first appearance in rocks of Triassic age; that they continue to be represented by a few small marsupial-like creatures up to the beginning of the Cretaceous period, where, with a single exception,* the record is lost until the Eocene is reached. It is likewise well-known that the fauna of this period is comparatively highly specialized and comes into existence, so far as we now know, without announcement in the preceding formation.

Previous to the discoveries of Professor Cope, the Wasatch was the oldest Eocene deposit of which we had any knowledge. These beds contain the remains of an extensive fauna, a large proportion of which is composed of Perissodactyle ungulates. They likewise contain Rodents, Lemurs, Creodonts, Taxeopods, etc.

In the present volume we have brought to our attention an Eocene fauna which antedates that of the Wasatch, viz: that of the Puerco. Here, so far, no Perissodactyles have been discovered; neither have Rodents as yet been found, although it appears to be quite rich both in species and individuals. Since these groups have always been hitherto regarded as a constant feature of any early Eocene fauna, this is somewhat remarkable.

The Perissodactyles appear to be replaced by a most extraordinary group to which an ordinal rank has been assigned by the author under the name *Taxeopoda*. The Rodents are represented by the *Tæniodonta*, an assemblage of extinct forms with large scalpriform incisors in both jaws, while Lemurs, Creodonts and primitive allies of the Coryphodonts go to make up the list of the mammals.

Altogether, the *facies* of this fauna is much more primitive than that of any other group of Eocene Mammalia so far known, and its discovery may be justly regarded as the most important that has been made in this subject within the past decade. To those who await with confidence the discovery of the connecting links between the mammals of the Jurassic and

* Messrs. Wortman and Hill discovered the remains of a small Stereognath marsupial in the Laramie deposits of Dakota in the summer of 1883, associated with the bones of huge Dinosaurs. To this species Cope gave the name *Meniscoëssus*.

the rich fauna of the Eocene as heretofore understood, the discovery of the Puerco constitutes a bold advance.

As regards the important additions to our knowledge of special groups, which this work contains, they are so numerous that it is impossible to mention more than a few of the leading ones in this connection. We have here presented for the first time any thing like a broad and comprehensive generalization of the relationships of the hoofed Mammalia, Assuming as a basis of understanding the fact that the limb structure has been all important to these animals in the struggle to survive, Professor Cope has divided them into four groups according to the degree of modification of the carpal and tarsal articulations.

He has shown that in all primitive ungulates the carpal and tarsal bones are serially arranged; that is to say, those of the proximal row are directly superimposed upon the corresponding elements of the distal set, a condition whose mechanical advantage in sustaining the weight of a bulky body in rapid movement, is much inferior to that of the higher types in which the carpal and tarsal bones interlock.

The four orders which he thus constructs are the *Taxeopoda*, *Amblypoda*, *Proboscidea* and *Diplarthra*. The first of these is the most primitive, being pentedactyle, probably semi-plantigrade, bunodont, and in many ways approaching the clawed orders. It attained its greatest development in the Puerco epoch, where it is represented by numerous species and genera. According to the views of Professor Cope, which seem to be unusually well-founded, this group forms the central stem from which the others have been derived, having as a cotemporary in the Puerco horizon the *Taligrada*, a sub-order, which establishes a close connection between it and the *Pantodonta* of the later Eocene.

It is here also that the Hyrax, that anomalous nondescript, for the first time finds fellowship, being at the same time the only living representative of this remarkable order. It appears to be an oversight on the part of Professor Cope that he did not detect the ancestral connections of the tree Hyrax with his *Meniscotheriidæ*, a family of the *Taxeopoda*. It is likewise somewhat questionable whether he is correct in giving the *Toxodontia* a position in this order.

With reference to the immediate connections between the *Diplarthra*, a group corresponding to the *Ungulata* of most authors, and the Taxeopoda, comparatively little has been made out; neither do we receive any additional light upon the direct ancestry of the Proboscidians in the present contribution. The internal arrangement of the *Taxeopoda* is thoroughly

considered and many genera and species described. The characters of the order are established principally upon unusually perfect skeletons of *Phenacodus primævus* and *P. Vortmani*, which were obtained by one of his collectors in the valley of the Big Horn in Wyoming Territory.

A fact of no small significance is ascertained with regard to the superior molar teeth of some of the Puerco representatives. They are shown to be tritubercular and are therefore the simplest pattern which is known to exist in any ungulate. From this the evolution of the teeth of all the *Ungulata* from a simple type appears to be demonstrated.

In the Perissodactyle division of the *Diplarthra* much is added to our knowledge. The suborder is for the first time divided into families whose exact limits are defined and the genera systematically arranged. The osteology of the four-toed equine representative, *Hyracotherium*, is thoroughly described, and an almost complete skeleton figured. The osteology of the Lophiodont genera, *Triplopus* and *Hyrachyus*, are also described from exceptionally perfect skeletons. As already indicated, the discovery of the *Taligrada*, a new suborder of the *Amblypoda*, constitutes the principal advance in our information respecting this division, nevertheless numerous genera and species related to the Coryphodonts as well as one extremely interesting genus *Bathyopsis*, near to *Loxolophodon*, are also described and figured.

Another generalization of scarcely less importance is that which relates to the arrangement of the clawed Mammalia. From the previous publications of Marsh and Cope we have been made acquainted with the remains of several extinct groups which display characters intermediate between those of orders now living. These are the *Tillodontia* of Marsh, which has both Insectivorous and Rodent affinities, the *Tæniodonta* of Cope, which seems to connect the Tillodonts with existing Edentates and the *Creodonta* of Cope, which apparently blend the modern *Carnivora* with the ancient *Insectivora*. These, together with all existing Insectivores as well as the Lemurs, are grouped into a single order under the name *Bunotheria* and their relationship defined.

This has appeared indeed necessary since the additional evidence which paleontology affords unquestionably demonstrates the close affinities of these groups and strongly suggests a community of origin. Only three of these divisions are found in the Puerco, and these are the *Creodonta*, which are nothing more than slightly specialized Insectivores, the *Tæniodonta* and the Lemurs. It is evident therefore that the Tillodonts, Edentates Bats, Carnivores and Rodents must have been derived from these three, and seeing that the latter are so closely related in

this epoch it would not do very great violence to the system to unite under this order an even greater number of these divisions than is done by Professor Cope. The origin of the *Carnivora* from the *Insectivora* through the specialized offshoot *Creodonta* is demonstrated beyond all peradventure, it seems to us, while many interesting and important facts have been discovered which throw a great deal of light upon the philogenetic history of the Dogs and Cats of modern times.

Although not indicated by Professor Cope, it seems in the highest degree probable that in his Insectivorous genus *Esthonyx*, we have the ancestor of the *Tillodontia*, which in turn gave origin to the *Toxodontia*.

Among the Lemuroids many new and interesting genera are added to the list, as well as much important information respecting them. Prominent among these is the description of the skull of *Anaptomorphus*, a remarkably specialized form for so early a period as the Wasatch from whose rocks it was derived. Others of scarcely less importance are described and figured.

Another discovery of importance, which is here recorded, relates to the probable ancestry of certain of the marsupials, including the very curious genera *Plagiaulax* and *Thylacoleo*. According to Professor Cope, *Ctenacodon* of Marsh, from the American Jurassic, is the ancestral type from which these Plagiaulacid Marsupials were derived. The line of development, as indicated by him, is as follows: *Ctenacodon*, *Plagiaulax*, *Ptilodus*, *Catopsolis* and *Thylacoleo*. *Ptilodus* and *Catopsolis*, from the Puerco Eocene, are the important links which have been added by Professor Cope, establishing not only an interesting fact of phylogeny, but adding at the same time another link to the chain between the Jurassic and Eocene Mammalia.

Altogether this ponderous volume forms one of the most substantial contributions to the subject which has ever been made, and certainly marks an epoch in the history of paleontological science. The genera and species considered are well systematized and defined, the descriptions clear and accurate, while the illustrations are for the most part well done. That which, however, will in all probability commend the work most to thoughtful students of paleontology, is the unusual grasp of its philosophic deductions which are in every way worthy of the marked ability of its author.

U. S. Army Med. Mus., Washington, D. C.
July 16, 1885.

Art. XL.—*Observations upon the Tertiary of Alabama;* by T. H. Aldrich.

In the following article, the results of a personal examination of the Tertiary of Alabama are given so far as is necessary to reply to the papers of Dr. Otto Meyer in the June and July numbers of this Journal, entitled "The Genealogy and the Age of the species in the Southern Old-tertiary."

The proof of the stratigraphical relationship, as worked out by Dr. Meyer, rests upon quotations from previous writers upon the subject, and upon a theory of descent and resemblance, which Dr. Meyer applies to the fossils of the different groups. There does not seem to be any positive statement in his articles that the superposition of the beds as given by him came under his actual observation, therefore I shall proceed by giving the stratigraphy at different points, and then some remarks upon his identifications of species.

The old town of Claiborne, Ala., is built upon what is locally known as "second bottom" of the Alabama River, a level sandy plain over a mile wide at this place; Jackson on the Tombigbee River, and Selma and part of Montgomery, both upon the Alabama River, are built upon the same terrace and present almost precisely the same topographical features as Claiborne.

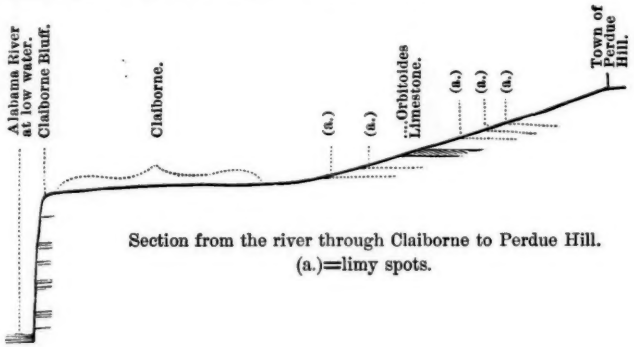
This terrace apparently is the oldest upon these rivers and has been subject to extensive erosion. The drift following filled up the depressions to a level plain, on the remains of which Claiborne now stands.

Going toward Perdue Hill through the main street of Claiborne (which runs in a southeasterly direction) at a point about one and a half miles back we reach the base of the hill; as we ascend it we find, in the road and gullies alongside, limy spots indicating the presence of the "White Limestone" below, and at a point about two miles from the bluff, on the side of the road, there is an outcropping of the White Limestone highly charged with Orbitoides and casts of several different forms of shells; by the aneroid barometer this point is 110 feet above the top of the bluff at the river. These limy spots continue to show still higher, with an occasional outcrop nearly to the top of the hill. The crown of the hill is covered with the red loam, and is 180 feet above the top of the bluff.

The dip of the strata at Claiborne seems to be a little west of south, therefore this street is very nearly upon the strike. This fact, together with the difference in level, plainly proves that at Claiborne itself the White Limestone is above the Claiborne group. The dip of the strata can be shown by

observations made by a careful writer, the Rev. C. S. Hale;* on page 359 of that article he states that the Claiborne sand (No. 7) appears at low water at a point four miles south; the bottom of the bed at the Claiborne bluff is about 90 feet above low water; this shows a southerly dip of at least 22 feet per mile. The true dip is more than this, as the river runs W.S.W. between these places.

The following section with the above explanation will give a clear view of the vicinity of Claiborne.



Section from the river through Claiborne to Perdue Hill.
(a.)=limy spots.

South of Claiborne where the road crosses Randon's Creek, I found fine specimens of *Pecten Poulsoni* Mort., *Orbitoides Mantelli* Con., a species of Echinus and several casts of other shells. All these species are also found at Perdue Hill.

The section of Claiborne bluff following shows (No. 9 and 10) strata not given by Dr. Meyer, probably owing to a higher stage of water at the time of his visit. There is otherwise no material difference. The measurements were made at the lower landing with the exception of the two lowest beds which rise up at the upper landing and above (another proof of a southerly dip).

I wish here to correct an error which seems to be made by nearly every writer who has given a profile of the bluff at Claiborne,† and that is the designation of the lower part of the Claiborne section as *limestone*. It is a calcareous clay and not limestone, the average proportion of lime being about 12 per cent,‡ and Winchell's statement that this bed has not been recognized elsewhere is also erroneous, as shown further on.

Beds Nos. 9 and 10 are particularly interesting from the fact

* Geology of South Alabama, by C. S. Hale, this Journal, II, vol. vi, pp. 354–363. 1848.

† Conrad. J. A. N. S., 1st Series, vol. vii, p. 122. 1834. Winchell, P. A. A. S., 1856, Part I, p. 86. Tuomey, 1st Bien. Rept., 1850, p. 152.

‡ Lea, Contrib. to Geol., 1833, p. 22. Mell, Tr. Am. Inst. M. Engrs., 1880.

Section of the Bluff at Claiborne, Ala.

	40 ft.	DRIFT.	
LOWER LANDING.	45 ft.	White Limestone bed, containing Scutella and casts of shells. Zeuglodon bones found in this bed.—Hale, this Journal, 1848, p. 361	D
	3 ft.	Scutella beds	C
	6 ft.	Coarse ferruginous sands, indurated at bottom	B
	17 ft.	Claiborne sand—thin layers of lignite show about 10 ft. down from top. In places this layer becomes over a foot thick	A
	4 ft.	Indurated sandy ledge	1
	18 ft.	Calcareous clayey strata becoming sandy in lower part	2
	1 ft. 6 in.	Indurated sandy ledge	3
	5 ft.	Calcareous clay sandy at bottom	4
	5 ft.	Light yellowish gray calcareous sand, lower part indurated containing casts of shells	5
	27 ft.	Light yellowish gray calcareous sands containing *O. sellæformis* Con., *Scutella Lyelli* in fragments, *Scalpellum Eocense* Mr., *Pecten Deshayesii* Lea, *Pecten scintillatus?* Con., etc. Shows indurated ledges in some places	6
	3 ft.	Layer of comminuted oyster shells	7
UPPER LANDING.	2 ft.	Dark blue black sandy clay	8
	10–15 ft.	Bluish green clayey sands, very few fossils in upper part, crowded below, a large number of the fossils distorted by pressure containing *O. selleformis.* Con., *Venericardia rotunda* Lea, *Nucula magnifica, Arca rhomboidella* Lea, *Anomia — n. s., Amphidesma limosa* Con. — many other bivalves	9
	6 ft.	Dark bluish greens and containing a peculiar small form of *Vener. planicosta* Lam., *Turritella Mortoni* Con., *Turritella —n. s. Crassatella—sp?, Corbula—sp?*, and many bivalves.	10

Alabama River at low water.

that they contain species in abundance that are rare in the Claiborne sand. *Turritella Mortoni* is found very large here. Owing to illness a complete list of species has not yet been prepared. The geographical distribution will be given in the future; the range of many species will be a surprise to those who rely upon paleontological evidence alone to prove the age of the various beds of the Tertiary of Alabama and Mississippi. In many cases actual superposition seems to be the only sure guide.

Now if Dr. Meyer's theory is correct we should find underneath the Claiborne beds the Jackson and Vicksburg formations.

We give below what we actually found. Proceeding up the river from the Claiborne Landing we lose all trace of the Tertiary rocks in about a mile and a half; then till we reach Lisbon, on the west side of the river about five miles up, only the recent bottom lands appear; here is a fine exposure of the lower Claiborne beds in a nearly vertical bluff about a mile long.

Section at Lisbon.

(*a*) Surface soil or loam	20 feet.
(1) Sandy strata with clay streaks, no fossils observed,	10 feet.
(2) Sandy clays, dark brown, badly weathered, highly fossiliferous, equals No. 9 and 10 of Claiborne section, contains some new species, also *Amphidesma limosa* Con., *Arca rhomboidella* Lea, *Turritella*, n. s., *V. planicosta* Lam., *V. rotunda* Lea, *Lucina compressa* Lea, *Ancillopsis vetustus* Con., *Rostellaria Whitfieldi* Heilpr., etc.	12′ 0″
(3) Hard sandy ledge	0 8″
(4) Calcareous clayey sands, light yellow when wet, nearly white when dry*	6′ to 8′ 0
(5) Coarse grained ferruginous sands, fossils numerous	3′ 0″
(6) and (7) Light yellow sand with a hard ledge on top, lower five feet dark blue when wet	20 feet.
(8) Bluish-black clay with remarkable fucoidal looking raised rib-like concretions upon the exposed bedding planes	8 feet.

No. 8 is the top of the Buhr-stone series which are exposed higher up the river. *Lapparia dumosa* Con.,=*Mitra pactilis* of Claiborne sand, is found in No. 5. This is the only Jackson fossil in addition to those already known found here.

About one mile from McCarty's ferry on the Tombigbee

* There is a remarkable difference in color between beds when wet and when dry. At Prairie Bluff, Ala., we noticed that deep blue clayey beds weather out higher upon the bluff, where perfectly dry, to a nearly pure white sand. The distinctive characters are so few between hundreds of layers that it is hard to characterize them so as to be recognized by a future observer.

River, we measured the Buhr-stone with the aneroid, making it 270 feet thick from our camp on top of the hill to the river. Dr. Smith's observations at other points indicate at least 70 feet more to go on top of this, giving at least 340 feet of thickness.

All the fossils found in the Buhr-stone were Claibornian in specific character; I mention *Venericardia parva* Lea, *T. obruta* Con., *V. rotunda* Lea and *Corbula Murchisoni* Lea.

Continuing our section downward we find at White Bluff on east bank of the Tombigbee River, the following, viz: at top.

(1) Hard Buhr-stone, vertical bluff	90–100 feet.
(2) Clays	30 feet.
(3) Clay with lignite stems distributed throughout	3′ 0″
(4) Clay	5′ 0″
(5) Lignite in fine streaks	0′ 6″
(6) Barren clays	100′ 0″

Coming up the river toward Wood's bluff, we find, rising from under this exposure:

(7) Clays, some layers very sandy. Found one specimen of *Athleta Tuomeyi* Con. here	50′ 0″
(8) A thin streak of fossiliferous sand containing Wood's bluff fossils	0′ 2″
(9) Clays, barren	12′ 0″
(10) 2–4 feet Green sand, brown outside, dark green when freshly cut, fossiliferous, *Lævibuccinum lineatum* Heilpr., seems confined to this bed	4′ 0″
(11) Clay, dark grayish-blue, containing *Athleta Tuomeyi* Con., abundant	8′ 0″
(12) Indurated greensand	2′–3′ 0″
(13) Greensand marl (fine fossils here)	3′
(14) Indurated Greensand marl crowded with shells, large proportion Turritellas	6′ 8″
(15) Indurated Greensand marl ledge making a shoal in the river, showing	5′ 0″

Nos. 12, 13, 14 and 15 really form one marl bed, the distinctions are mainly in hardness and in being more or less fossiliferous. This group (Wood's bluff) is very extensive, and can be traced easily to the western border of the State. We found it at Butler, Choctaw Co. Professor Heilprin has described most of the species found here.*

It is unnecessary to continue this series farther to prove the object in view; but nearly every bed down to the Cretaceous has been examined without revealing the Jackson or Vicksburg groups.

Dr. E. A. Smith and Dr. Lawrence Johnson have made sec-

*P. A. N. S., 1880, p. 364.

tions of the whole Tertiary. Their report is now in course of publication by the National Survey.

Having personally collected, in company with Dr. Smith this summer, in nearly every bed known below this (Wood's bluff) horizon, I can testify that no evidence of much value can be obtained from the fossils. The Vicksburg and Jackson have not been found down 710 feet below Claiborne as the above sections will testify; if they are below this, then Dr. Meyer has got to sandwich in between Claiborne and Jackson the whole of the Buhr-stone formation. This formation is found only in the N.E. of Mississippi, a long distance from the position of the two groups mentioned.

Dr. Meyer is equally unfortunate in his quotations from authorities. I review as follows:

1833. Conrad, *Spondylus dumosus* Mort. is here spoken of as a stumbling block in Conrad's way and especially to Lyell. We have lately found it at Hatchitigbee bluff, 25 feet beneath the buhr-stone.

1834. Observations, etc., Conrad. In this section Conrad distinctly states that he saw only the "White Limestone" and the "Bluish Limestone," Nos. 7 and 8 of his section *near* Claiborne, and probably made his erroneous determination from the fact, as stated by Lyell, that the Claiborne beds are worn away largely in places and have been replaced by the "White Limestone." If Conrad had made a trip to Perdue Hill at the time of his visit the error would not have been made.

March, 1846. Lyell's general statements are correct and proved by all subsequent observers to have been very carefully made.

1850. M. Tuomey, 1st Biennial Rept. of the Geol. of Ala. Dr. Meyer quotes Tuomey as follows, p. 149: "Sir Chas. Lyell has proved that the White Limestone is newer than the fossiliferous bed at Claiborne by showing that this bed which contains identical fossils underlies the bluff at St. Stephens. This is certainly the case, for although this bed is not seen at the base of the bluff it is overlaid, as I have just stated, by a yellow limestone which is a prolongation of that at St. Stephens."

By a juxtaposition of sentences Dr. Meyer evidently proves satisfactorily to himself "that Lyell's Claibornian bed at the base of St. Stephens bluff according to his (Lyell's) determination need not be Claibornian, but that it is also *not* at the base of St. Stephens bluff." Having personally examined this exposure within the past month in company with Professor Smith, State Geologist of Alabama, I consider that Lyell's statement is correct. The same bluff that is at St. Stephens is over the Claiborne sand bed at the point Tuomey speaks of. A rough section taken at this place, which was hurriedly done owing to a heavy shower at the time of our visit, is as follows:

Section half a mile north of St. Stephens.

No. 6. White limestone, highly charged with *Orbitoides Mantelli*..... 40 feet.

No. 5. Hard ledge limestone containing spines of a species of Cidaris..... 2 feet.

No. 4. White limestone containing *P. perplanus* Mort., etc., no Orbitoides found,..... 50 feet.

No. 3. Scutella bed. Yellow sand indurated in places,.. 2 feet.
Also containing Osteodes — sp. ?

No. 2. Claiborne sand, 15 feet thick in places measured, containing the well known Claiborne fossils..... 15 feet.
The sand is a trifle redder than at Claiborne, but a single glance is enough to show they are the same. I noted the following species among many others : *Rostellaria velata* Con., *Pecten Deshayesii* Lea, *Crassatella protexta* Con., *Venericardia rotunda* Lea, *V. transversa* Lea, *Turbinolia Maclurii* Lea, *Dentalium thalloides* Con., *Astarte sulcata* Lea, *Corbula Murchisonii* Lea, *Cytherea perovata* Con., *Cyth. æquorea* Con., *Fusus protextus* Con., *Melongena alveata* Con., *Crassatella alta* Con.

No. 1. A blue sandy clay containing a few Claiborne fossils and a species of Osteodes..... 10 feet.
Tombigbee River level.

This should be convincing.

Dr. Meyer in his opening quotation from Tuomey which is given previously should have added the succeeding paragraphs. I quote:* "Another locality occurs a few miles from Clarksville on the land of Mr. Chambers, on one of the branches of Satilpa Creek, where this fossiliferous (Claiborne) bed is laid bare by the denudation of the upper beds and appears in the bottom of a ravine, in the very midst of the White Limestone, at a locality too where the latter rock is rich in the remains of Zeuglodon." Dr. M. leaves out of his authorities the Rev. C. S. Hale.† The reader is referred to p. 360 where he gives two localities near Claiborne (below) showing a section from the Scutella bed (C. of my Claiborne profile) up to the Orbitoides limestone.

Winchell,‡ on page 84–85, gives two localities where the Claiborne sand has the White Limestone above it, namely, Stone Creek and in Clark County, Ala.

The northern dip mentioned by Hilgard has its counterpart in Alabama in several places; in fact there is a large basin in the Tertiary of Alabama, first spoken of by Tuomey,§ probably with a smaller sub-basin north of it. Professor E. A.

* 1st Bienn. Rept., 1850, p. 148.
† This Journal, 1848, p. 354–363.
‡ P. A. A. S., 1856, pp. 82–93.
§ 1st Rept., 1850, p. 150, and in Hilgard. this Journal, new series, 1867, vol. xiii, p. 37.

Smith has confirmed this, and is now engaged in completing his observations.

A locality north of Barrytown, Ala., at and in the vicinity of a mill spoken of by Tuomey,* furnishes another section from the Lisbon beds up to the White Limestone, a small patch of which is left on top of Womac hill. The bed at the mill contains: *O. sellæformis* Con., very large and fine; *Teredo* — n. s.; *Scalpellum Eocense* Meyer; *Pecten Deshayesii* Lea, and corresponds with the bed (*b*) of Dr. Meyer's section. Winchell's statement that this bed is not found elsewhere is disproved.

The species described by Conrad† which were received from Dr. Spillman in which he gives the locality "Enterprise, Miss.," Dr. S. writes me (Aug. 14, 1884) were not found there; he says: "I have no recollection of sending T. A. Conrad any fossils from near Enterprise. I sent him some from Garland's Creek, three miles east of Shubuta, Miss., in the southern part of Clark County." This removes one question, as these shells are undoubtedly Jacksonian and Dr. Meyer is no doubt correct in calling the beds at Enterprise Claibornian.

One more point remains to be quoted, and this is, that the crystalline limestone of the Vicksburg group has never been found in Alabama below the Claiborne sand, while crystalline limestone over 150 feet thick shows above it.

Reviewing Dr. Meyer's summary of reasons:

1st. He speaks of the lower limestone, which is not a limestone; also sections already given show beyond question that Lyell was correct in his general statements.

2d. This is no argument whatever; differences in level of 100 feet between points nearly 100 miles apart have no stratigraphical value. If the general dip is southerly in Alabama and southwest to west in Mississippi, all the beds are sure to outcrop at the surface *somewhere.*

3d. The Jackson group in Mississippi presents a mixture of Vicksburg and Claiborne forms, and this very fact is a strong argument in favor of its true position, being between the two groups. *Mitra pactilis* Con. is common at Jackson as *M. dumosa* Con., yet it also is found at Lisbon, still lower than Claiborne sand by 100 feet. The relationship is therefore to be largely widened.

4th. *V. parva* Lea has a still larger range, probably through 1000 feet of strata.

5th. *Venericardia diversidentata* Mr., from Jackson, is nothing more nor less than *V. rotunda* Lea. Conrad at one time evidently considered it new, as he gives a name in Wailes (Geol. of Miss.) "*Cardita tetrica,*" but afterward abandons it. This

* 1st Rept., p. 148. He also mentions finding *O. Sellæformis* Con. here.
† Am. Jour. of Conchology, 1865, vol. i, p. 137.

species ranges through nearly the whole Tertiary, the Vicksburg group included. I have it from Vicksburg.

Fulgur Mississippiensis Con. and *Tellina Vicksburgensis* Con. are found in both groups, also in the intermediate Red Bluff strata. Why they could not *ascend* from the Jackson to Vicksburg passes my understanding. *Fulgur filius* Mr. seems from the description to be *F. Mississippiensis* itself; the differences pointed out are trifling, and there are intermediate forms.

6th. *Pleurotoma terebralis* Lam. = *P. cristata* Con. I have from the Greggs Landing marl which is several hundred feet below the Wood's bluff group and not far from the Cretaceous, therefore its origin is below all the groups in question. The Acteon spoken of has a greater range than given.

In reference to *Natica Mississippiensis* Con.: As the Wood's bluff is over 700 feet below the Claiborne its origin is simply shown to be below the three groups under discussion.

7th. Here Dr. Meyer is guilty of assuming a parallelism which he has not *seen* and of which he gives no *proof* whatever.

In conclusion, let me state that the Tertiary is a great deal thicker than has been before supposed, and arguments based upon the upper quarter of its thickness are very likely to be upset by the paleontology of the three-fourths as yet unknown. An enormous territory remains here for the paleontologist, both in Mississippi and Alabama, entirely outside of the three groups, Vicksburg, Jackson and Claiborne, full of beautiful new species and gigantic forms of little Claiborne shells, that would cause one to exclaim, what could have led to such degeneration!

There is no doubt that there is a relationship existing between fossil species the same as in living forms, but, until the great unknown territory is more fully explored, comparisons are not apt to be of much value. Dr. Meyer has shown great industry in his papers, and apart from his unfortunate mistake in stratigraphy, they are well worth especial study.

ART. XLI. — *On the Electrical Furnace and the reduction of the Oxides of Boron, Silicon, Aluminum and other metals by Carbon;* by EUGENE H. COWLES, ALFRED H. COWLES and CHARLES F. MABERY.*

THE application of electricity to metallurgical processes has hitherto been confined chiefly to the reduction of metals from solution, and few attempts have been made to effect dry reductions by means of an electric current. Sir W. Siemens endeavored to utilize the intense heat of an electric arc for this purpose, but accomplished little beyond fusing several pounds of steel. A

* Read at the Ann Arbor meeting of the American Association.

short time since Eugene H. Cowles and Alfred H. Cowles, of Cleveland, conceived the idea of obtaining a continuous high temperature on an extended scale by introducing into the path of an electric current some material that would afford the requisite resistance, thereby producing a corresponding increase in the temperature. After numerous experiments that need not be described in detail, coarsely pulverized carbon was selected as the best means for maintaining a variable resistance, and, at the same time, as the most available substance for the reduction of oxides. When this material, mixed with the oxide to be reduced, was made a part of the electric circuit in a fire-clay retort and submitted to the action of a current from a powerful dynamo machine, not only was the reduction accomplished, but the temperature increased to such an extent that the whole interior of the retort fused completely. In other experiments, lumps of lime, sand and corundum were fused, with indications of a reduction of the corresponding metal; on cooling, the lime formed large well defined crystals, the corundum beautiful red, green and blue hexagonal crystals.

Following up these results with the assistance of Charles F. Mabery, Professor of Chemistry in the Case School of Applied Science, who became interested at this stage of the experiments, it was soon found that the intense heat thus produced could be utilized for the reduction of oxides in large quantities, and experiments were next tried on a large scale with a current from two dynamos driven by an equivalent of fifty horse-power. For the protection of the walls of the furnace, which were made of fire-brick, a mixture of the ore and coarsely pulverized gas carbon was made a central core, and it was surrounded on the sides and bottom by fine charcoal, the current following the lesser resistance of the central core from carbon electrodes which were inserted at the ends of the furnace in contact with the core. In order to protect the machines from the variable resistance within the furnace, a resistance box consisting of a coil of German silver wire placed in a large tank of water was introduced into the main circuit, and a Brush ammeter was also attached by means of a shunt circuit to indicate the quantity of current that was absorbed in the furnace. The latter was charged by first filling it with charcoal, making a trough in the center and then filling this central space with the ore mixture, which was covered with a layer of coarse charcoal. The furnace was closed at the top with fire-brick slabs containing two or three holes for the escape of the gaseous products of the reduction, and the entire furnace made air-tight by luting with fire-clay. Within a few minutes after starting the dynamo, a stream of carbonic oxide issued through the openings, burning usually with a flame eighteen inches in height.

The time required for complete reduction was ordinarily about an hour.

The furnace at present in use is charged in substantially the same manner, and the current is supplied by a Brush machine of variable electromotive force driven by an equivalent of forty horse-power. A Brush machine capable of utilizing 125 horse-power, or two and one-half times as large as any hitherto constructed by the Brush Electric Company, is being made for the Cowles Electric Smelting and Aluminum Company, and this machine will soon be in operation. Experiments already made show that aluminum, silicon, boron, manganese, magnesium, sodium and potassium can be reduced from their oxides with ease. In fact there is no oxide that can withstand temperatures attainable in this electrical furnace. Charcoal in considerable quantities is changed to graphite; whether this indicates fusion or solution of carbon in the reduced metal has not been fully determined. As to what can be accomplished by converting enormous electrical energy into heat within a limited space, it can only be said that it opens the way into an extensive field for pure and applied chemistry. It is not difficult to conceive of temperatures limited only by the capability of carbon to resist fusion. The results to be obtained with the large Brush machine above mentioned will be of some importance in this direction.

Since the cost of the motive power is the chief expense in accomplishing reductions by this method, its commercial success is closely connected with the cheapest form of power to be obtained. Realizing the importance of this point the Cowles Electric Smelting and Aluminum Company has purchased an extensive and reliable water-power, and works are soon to be erected for the utilization of 1200 horse-power. An important feature in the use of these furnaces, from a commercial standpoint, is the slight technical skill required in their manipulation. The four furnaces in operation in the experimental laboratory at Cleveland are in charge of two young men 20 years of age who, six months ago, knew absolutely nothing of electricity. The products at present manufactured are the various grades of aluminum bronze made from a rich furnace product that is obtained by adding copper to the charge of ore, silicon bronze prepared in the same manner, and aluminum silver, and alloys of aluminum with several other metals. A boron bronze may be prepared by the reduction of boracic acid in contact with copper.

As commercial results, may be mentioned a daily production in the experimental laboratory averaging fifty pounds of 10 per cent aluminum bronze; and it can be supplied to the trade in large quantities at prices based upon $5 per pound for

the aluminum contained, the lowest market quotation of this metal being at present $15 per pound. Silicon bronze can be furnished at prices far below those of the French manufacturers.

The alloys, which the metals obtained by the methods above described form with copper, have been submitted to careful study. An alloy containing 10 per cent of aluminum and 90 per cent of copper forms the so-called aluminum bronze, with a fine golden color, that is retained in the atmosphere for a long time. The tensile strength of this alloy is usually given as 100,000 pounds to the square inch; but castings of our 10 per cent bronze have stood a strain of 109,000 pounds. It is a very hard, tough alloy, with a capacity to withstand wear far in excess of any other metal in use. All grades of aluminum bronze make fine castings, taking very exact impressions, and there is no loss in remelting as in the case of alloys containing zinc. The 5 per cent aluminum alloy is a close approximation in color to 18 carat gold and does not tarnish readily. Its tensile strength in the form of castings is equivalent to a strain of 68,000 pounds to the square inch. An alloy containing 2 or 3 per cent aluminum is stronger than brass, possesses greater permanency of color and would make an excellent substitute for that metal. When the percentage of aluminum reaches 13 an exceedingly hard, brittle alloy of a reddish color is obtained; and higher percentages increase the brittleness and the color becomes grayish-black. Above 25 per cent the strength again increases.

The effect of silicon in small proportions upon copper is to greatly increase its tensile strength. When more than 5 per cent is present the product is exceedingly brittle and grayish-black in color. It is probable that silicon acts to a certain extent as a fluxing material upon the oxides present in the copper, thereby making the metal more homogeneous. On account of its superior strength and high conductivity for electrical currents, silicon bronze is the best material known for telegraph and telephone wire.

The element boron seems to have almost as marked an effect upon copper as carbon does upon iron. A small percentage in copper increases its strength to 50,000 or 60,000 pounds per square inch without diminishing to any extent its electrical conductivity.

Aluminum increases very considerably the strength of all metals with which it is alloyed. An alloy of copper and nickel with a small percentage of aluminum, called Hercules metal, withstood a strain of 105,000 pounds and broke without elongation. Another grade of this metal broke under a strain of 111,000 pounds with an elongation equivalent to 33 per cent. It must be remembered that these tests were all made upon castings of the alloys. The strength of common brass is doubled by the addition of 2 or 3 per cent of alumi-

num. Alloys of aluminum and iron are obtained without difficulty; one product was analyzed containing 40 per cent of aluminum. In the furnace, iron does not seem to be absorbed readily by the reduced aluminum when copper is present; but in one experiment a mixture composed of old files 60 per cent, nickel 5 per cent, and of 10 per cent aluminum bronze, 35 per cent was melted together and it gave a malleable product that stood a strain of 69,000 pounds.

When the reduction of aluminum is conducted in the absence of other metals it forms a compound with carbon analogous to pig iron as it comes from the blast furnace; and products are frequently analyzed that contain sixty or seventy per cent of aluminum. If the ore contains silicon the latter is absorbed by the aluminum and compounds of the two elements containing ten or fifteen per cent of silicon are often taken from the furnace in considerable quantities. These important products are at present under examination.

ART. XLII.—*The Grand Rapids Meteorite;* by R. B. RIGGS.

IN a recent number of this Journal (October, 1884), I. R. Eastman describes a meteorite found in Grand Rapids, Michigan. A preliminary analysis was made at the time, but of a very inadequate amount of the oxidized material, taken from the surface. Since then the meteorite has come into the keeping of the National Museum, and a more complete analysis gives the following results:—

Fe	88·71
Ni	10·69
Cu	·07
Mg	·02
P	·26
S	·03
C (combined)	·06
Graphite	·07
	99·91

It is a mass of great apparent homogeneity, weighing originally about 50 kilograms. One of the sections, however, on being polished, discloses a nodule about a centimeter in diameter, like troilite in appearance, which remains to be investigated.

A polished surface of the meteorite etched with nitric acid developed very handsome Widmannstättian figures somewhat like those on the iron from Robertson County, Tennessee.

Chemical Laboratory U. S. G. S., Washington, Aug. 26, 1885.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Sensitiveness of Selenium and Sulphur to Light.* — The remarkable property possessed by selenium of having its electrical resistance varied by the action of light has been the subject of many investigations, since its first announcement in 1873. The cause of this phenomenon is discussed by Shelford Bidwell in a recent number of the Philosphical Magazine (August, 1885, pp. 178–191). Remarking upon the ingenious method of forming selenium cells described by C. E. Fritts, who melted the selenium as a thin film on a plate of metal with which it forms a sort of chemical combination, he suggests the similarity of some of the phenomena described by Fritts to those of electrolysis. The arrangement of the two metallic plates with the third substance between them, that is, of the selenium between the metal plate upon which it is melted and the gold leaf film by which it is covered, is suggestive of this; while the unequal resistance of the two surfaces and the generation of an independent electromotive force, in conjunction with the polarization effects observed, make the conduction of selenium seem truly electrolytic. The objection that the selenium itself is not a electrolyte is met by the suggestion that in the process of making the cell a metallic selenide may have been formed, and the apparently improved conductivity of the selenium, and the accompanying phenomena, may be accounted for by the existence of this selenide, rather than by any change in the crystalline condition of the selenium.

As bearing upon this question the author made a series of experiments with sulphur. Five parts of sublimed sulphur and one of precipitated silver were heated together, and a cell constructed by spreading some of the melted sulphur, containing particles of silver sulphide, evenly over a surface of mica, and then laying a piece of thin silver-foil on its surface. The cell was found to vary in resistance to a marked degree when exposed to the light of a burning magnesium wire. Other experiments went to show the same result and to prove that the effect of the light was due to the action of radiation proper and not to any incidental rise in temperature. Other cells, constructed in a somewhat different manner, behaved in the same manner though in different degrees; with all the resistance diminished to a marked extent under the action of radiation. One of them, the resistance of which was small, was connected with ten Leclanché cells and a telephone, and exposed to a rapidly interrupted beam of light; it gave a musical note nearly as loud as would be obtained from a good selenium cell. All of the sulphur cells resembled selenium in giving polarization-currents after being detached from the battery. The author urges that the effects of radiation, in the case of the sulphur-silver cell, are to be looked

for principally at the surface of the electrodes, though not necessarily confined entirely to it. It is shown that light favors the union of sulphur and silver to form silver sulphide, and it is suggested that the same influence which would assist the union of two substances when they have a tendency to unite might act conversely when they have a tendency to separate. Radiation, therefore, acting upon a thin layer of silver sulphide might exert a material influence upon the conductivity of the sulphide by facilitating the molecular rearrangement of the atoms of sulphur and silver. The bearing of all this upon the explanation of the action of light on selenium is obvious, though experimental proof is needed to establish it.

2. *Molecular Shadows in Incandescent Lamps.* — The formation of metallic deposits in incandescent lamps with a carbon filament is described by J. A. FLEMING, the metal coming from the wire to which the carbon is attached. A sage-green deposit of copper is observed in an Edison lamp not infrequently; and in one case a silvery transparent metallic film of platinum was deposited on the inside of the glass bulb. These deposits recall the experiments of Wright (this Journal, xiii, 49; xiv, 169, 1877), who formed brilliant specula by deposition from a metallic electrode in exhausted tubes. An interesting point in the lamp deposits was the existence, under some circumstances, of a line of no deposit on the surface in the plane of the filament. This is to be regarded as the shadow of the loop in which the trajectory of the molecules is interfered with by the carbon filament. The deposit near the neck of the bulb is thicker than at the crown because of the greater number of molecules which here strike the glass. In the case of the carbon deposit, often observed, it was found that the molecular shadow of the filament, or the line of no deposit, could be formed by suddenly raising the filament to a very high temperature, as for instance by placing a 50-volt lamp for an instant on a 100-volt circuit; but when the deposit went on slowly no line was observed, only a general smokiness. In the former case the projection of the molecules from the carbon is violent enough to prevent their paths from being altered by collision with molecules of the residual air, so that a larger proportion reach the envelope in the direction of projection, thus causing a deposit on all parts except as shielded by the loop. The best shadows were obtained from a lamp with a single loop.—*Phil. Mag.*, August, 1885, pp. 141–144.

3. *Disintegration of the carbon filament in an incandescent Electric lamp.*—Some experiments carried on by BUCHANAN, having as their object the deciding of the question as to whether the observed breaking of the carbon filament at the negative end was due to a local excess of temperature or a "Peltiér effect," led to a negative result; the local heating detected being too small to account for a break at one end rather than the other. The author concludes that the disintegration of the carbon filament preceding complete destruction, as described by Preece, is doubt-

less the result of molecular changes in its structure produced by maintaining it at very high temperature; and he finds manifestation of the alteration in the change of thermo-electric power of the carbon.—*Phil. Mag.*, August, 1885, pp. 117–126.

4. *On changes produced by Magnetization in the Length of Rods of Iron and Steel.*—An abstract of an article on this subject by SHELFORD BIDWELL, after mentioning the results reached by earlier experimenters, goes on to remark that by using thinner iron rods and greater magnetizing forces the curious fact was established that if the magnetization be carried beyond a certain critical point, the consequent elongation, instead of remaining stationary at a maximum, becomes diminished, the diminution increasing with the force. If the force is sufficiently increased, a point is reached where the original length of the rod is totally unaffected by magnetization; and if the magnetization be carried still further, the original length of the rod is reduced. It also appeared that the position of the critical point in steel depended in a remarkable manner upon the hardness or temper of the metal. The author embodies his results for iron, steel and nickel in a series of formal laws.—*Proc. Roy. Soc.*, No. 237, p. 265.

II. GEOLOGY AND MINERALOGY.

1. *Notes on some of the Geological Papers presented at the Meeting of the American Association at Ann Arbor:* *

(1.) A. WINCHELL. On the Geology of Ann Arbor. The following is a summary of the geological facts within easy reach of Ann Arbor. Drift covers the nearest outcrop of rock 20 miles away; depth of drift at the University 200 feet, at the Observatory, 230 feet; on hills west and north probably 375 feet. Indications of terminal moraine. Kettle hole within a few rods. The drift accumulations rest on the outcropping belt of Marshall sandstone, trending northeast-southwest. Sheets of impervious materials included, forming water basins, and this source of supply is the basis of the water-works of Ann Arbor. Tabular limestone masses imbedded in upper part of drift as formerly described by the writer in this Journal, II, xl, 331–8; ascribed to action of ice-floes in Scientific Monthly, Toledo, Oct., 1875, this Journal, III, xi, 225–8; with additional facts in Proc. Amer. Assoc., xxiv, 1875, pp. 27–43; differently explained by T. C. Chamberlin in Ann. Rep. U. S. Geol. Surv., 1881–2, pp. 326–330.

(2.) S. G. WILLIAMS. Traced the Lower Helderberg strata into western New York.

(3.) A. H. WORTHEN. On the Quaternary deposits of Illinois. The author exhibited sundry sections obtained by borings in central and southern-central Illinois. They showed generally that the lower portion of the Quaternary formation is strictly stratified; that no bowlder beds exist, but that bowlders are sparsely scattered through the middle portions; that a dirt bed generally

* For these notes this Journal is indebted to Professor Alexander Winchell.

separates the lower and middle portions; that finally, the explanations usually applied to the northern drift will not apply in these parts of Illinois.

(4.) A. WINCHELL. On sources of trend and Crustal Surplusage in mountain structure. [The first part embodied views embraced in a paper sent Professor J. D. Dana in 1881.] The second part traced the consequences of slow subsidence of the earth's equatorial protuberance resulting from the secular retardation of its axial velocity of rotation caused by the action of the moon on the lagging tide. [The section voted to request a fuller abstract of this communication, the paper being orally presented.]

(5.) WM. B. TAYLOR. On a probable cause of the shrinkage of the Earth's Crust. By a singular coincidence this paper considered the second cause mentioned in the preceding paper. It was, however, only to explain *surplusage* of circumference; while the other employed the principle for both surplusage and trend.

(6.) H. S. WILLIAMS. On the classification of the Upper Devonian. Presented numerous studied sections of strata ranging east-and-west in southern New York, and pointed out the progressive changes in the faunas. He thought there was some ground for admitting that the equivalent of the Catskill group might be sought within the range of the Waverly series of the West. To this Professor Hall sharply demurred, and claimed that if the Catskill is made Carboniferous, then all, to the bottom of the Corniferous, must be so made, since traces of a Catskill fauna are found in eastern New York below the Corniferous. A. Winchell recalled the doctrine of "Colonies" maintained by Barrande, and instanced the case of an Upper Silurian fauna of over 3,000 feet occurring in the midst of the Lower Silurian in Bohemia.

(7.) EDWARD ORTON. Exhibited the records of a deep well at Cleveland, Ohio, in which over 200 feet of rock-salt were passed, with a parting of 15 feet of shale and 81 of limestone, at a horizon apparently *below* the Niagara limestone. But the final interpretation of the section was left for future study.

(8.) E. W. CLAYPOLE. On the Materials of the Appalachians. Held that the vast volume of the deposits and their increasing coarseness toward the southeastern part of Pennsylvania imply the former existence of a lofty pre-paleozoic range to the east of the present Appalachians.

(9.) N. H. WINCHELL. On *Lingula* and *Paradoxides* from the Red Quartzites of Minnesota. Exhibited a large slab from the "pipestone quarries," which was covered with small shells named by him *Lingula*. The remains of the shells on chemical testing showed a distinct phosphatic reaction. From the same quarries was exhibited a form regarded as an imperfect *Paradoxides*, showing the central axis and part of the pleuræ of the right side. As this pipestone bed is included in the quartzite of Wisconsin (at Baraboo, etc.) described by the Wisconsin geologists as Huronian, the discovery is important.

A. Winchell stated that the so-called shells appeared to be

undoubtedly brachiopods of the pleuropygian order, but with characters intermediate between *Lingulidiæ* and *Discinidæ*. He reminded the section that he had years before assigned the Baraboo quartzite to the Lower Silurian on lithological grounds, though Professor Irving had subsequently attempted to invalidate the reference. E. W. Claypole thought the objects exhibited were undoubtedly organic and coincided as to the affinities of the shells.

(10.) A. WINCHELL. On *Cœnostroma* and *Idiostroma* and the comprehensive character of Stromatoporoids. Enumerated the structural characters found in the group, and traced the morphology of each structure separately, showing on what grounds generic distinctions had been based. He then indicated the assemblages of characters which distinguish *Cœnostroma* and *Idiostroma* from other genera, and gives them validity. He finally pointed out the fact that the Stromatoporoids possess a very wide range of affinities: with *Sponges*, in external aspect, curdled tissue, water canals and oscula, though none of these characters are present in all; with *Foraminifera*, in their laminæ and interspaces; in their radial, branching canals, seen in Eozoön, and in the concentric layers, interspaces and curdled structures of foraminiferal *Parkeria;* with *Anthozoa*, in their laminæ (compared with *Heliolitidæ* and some *Favositidæ*), their radial structures, their individuation (incipient in *Cœnostroma*, completed in *Idiostroma*), and the (by some) supposed tabulæ of *Caunopora;* with *Hydrozoa*, in the resemblances of *Hydractinia* and more strikingly, *Labechia*, which may be regarded really a Stromatoporoid; with *Polyzoa*, in the tubes and (supposed) tabulæ of *Caunopora*, and some further resemblances of *Eschara nobilis*. It is vain, therefore, to seek to place Stromatoporoids within the bounds of any recognized class-type. The author exhibited numerous specimens, thin sections and photographs illustrating generic distinctions, and circulated a printed synopsis of Stromatoporoids.

For a full list of the geological papers presented to the Association see page 324.

2. *Can underground heat be utilized?*—J. STARKIE GARDNER has an article on this subject in the Geological Magazine for September. The author concludes from the increase of heat downward, from metamorphism, from volcanoes, and from the earth's up and down movements, that there is abundant reason for believing that the earth has a thin crust. The movements would be physically impossible in an earth solid throughout. If the principal mass is kept solid at a temperature beyond the fusing point of rock through the pressure of the external envelope, "the pressure must become relaxed as the surface is neared, and at a certain point the rock must obey its impulse and melt," and thus make a layer in a state of fusion. The movements of the crust are more compatible with a crust of ten miles thickness than with one of fifty miles. "The deepest artesian well in the world

is being bored at Pesth, and has already a depth of 951 meters. The work is undertaken by the brothers Zsigmondy, partially at the expense of the city which has granted £40,000 for the purpose, with the intention of obtaining an unlimited supply of warm water for the municipal establishments and public baths." The present temperature is 161° F.; and it will be prosecuted until water of 178° is obtained. About 175,000 gallons of warm water stream out daily, rising to a height of 35 feet. "It needs no seer to pierce the not-distant future when we shall be driven to every expedient to discover modes of obtaining heat without the combustion of fuel, and the perhaps far more remote future when we shall bore shafts down to the liquid layer and conduct our smelting operations at the pit's mouth."

3. *A gigantic bird of the Lower Eocene of Croydon, Gastornis Klaassenii;* E. T. NEWTON. (Geol. Mag., August, 1885.)—The remains of this bird indicate a species as large as the New Zealand Dinornis. The most perfect tibiotarsus when complete must have had a length at least of 20 inches, and its trochlear extremity is 3½ inches wide; while in another specimen the latter is 4 inches wide. The remains are from the "Blue Clay" and lignite patches of the Woolwich beds. The original specimen of Gastornis—*G. Parisiensis*—was from the Lower Eocene beds of Meudon, near Paris. The Anserine affinities of Gastornis, as regards the tibiotarsus, held by some writers, are confirmed by the detailed comparison of the Croydon bones with recent forms. In other parts of its organization the genus is regarded by Dr. Victor Lemoine as having affinities with the Ratitæ.

4. *Comstock Mining and Miners;* by ELIOT LORD. U. S. Geol. Survey, Clarence King, Director. 452 pp. 4to. Washington, 1883. (Recently issued; bearing the date of March 1, 1882, in the letter of transmittal.)—This report is a history of the development of the Comstock mines to the close of the year 1880, and, as the preface observes, it is the story of the birth of the silver-mining industry in this country as well as of its vigorous growth. On account of the great productiveness of the lode, the rapid movement in population it occasioned, the quick succession of events, and the later decline and depopulation, the history has unusual social and political interest. It is full of surprising incidents, and of vivid descriptions of scenes and occurrences, and contains much in the way of social and mining statistics. The interesting volume is illustrated by three excellent maps.

5. *Materialien zur Mineralogie Russlands von* N. VON KOKSCHAROW. Vol. ix, pp. 81–272. St. Petersburg, 1885.—A continuation of Koksharow's great work on Russian Mineralogy is always a welcome and valuable addition to mineralogical literature. The species discussed at length in this part of the 9th volume are turquois, wulfenite, topaz, vesuvianite, nepheline, sanidine, linarite.

III. Botany.

1. *The Microscope in Botany: a Guide to the Microscopical Investigation of Vegetable Substances. From the German of Dr. Julius Wilhelm Behrens.* Translated and edited by Rev. A. B. Hervey, A.M., assisted by R. H. Ward, M.D., F.R.M.S. Illustrated by thirteen plates and 153 cuts. Boston: S. E. Cassino & Co. 1885. pp. 466, 8vo.—This is a large and full book, on the microscope and its use in the investigation of vegetable structures and products. For the translator, while it has evidently been a labor of love, it must have been a long and serious task; and the publisher has brought it out in the best style, one would say upon superfluously fine and thick paper, which, however, allows the illustrations to appear at their best. One-third of the volume is devoted to the microscope and its appliances. The preparation of microscopical objects and microscopical reagents are discussed in about the same number of pages; and the microscopical investigation of the principal vegetable substances is treated with similar fullness. Dr. Ward has borne a part in the earlier chapters. Dr. Corwentz of Danzig contributed the short and very useful section upon the preparation of fossil plants. There is a good section on drawing under the microscope. In respect to the more important vegetable substances copious bibliographical references are appended. The microscope in this country is in many hands, and there is an increasing disposition to turn it to real scientific account;—for which this volume should be helpful. A. G.

2. *Bulletin of the California Academy of Sciences*, San Francisco.—The new departure made by the issue, in February, 1884, of the first number of this Bulletin, has been followed up with spirit by the publication, last February, of the still ampler No. 3, pp. 61–177, in direct continuation of No. 1. A No. 2, if it exists, is therefore out of pagination, and we believe was only a fly leaf. And now, in September, we receive No. 4, or at least the first part of it, continuing the volume to p. 228. The papers are all botanical, except two short ones by Dr. Behr on *Lepidoptera*. Dr. Harkness, still zealously devoted to the mycology of the Pacific coast, here gives us only a few pages, noting additional known Fungi and characterizing some new ones:—among them his *Lycoperdon sculptum*, well said to be "a curious and strikingly beautiful species," having a singular tuberculated cortex, the like of which has never been seen before. Plate I. gives a good representation of it.

Mrs. Curran, the efficient curator of the botanical collections at the Academy (which, happily, are at length being well cared for and in the way, as they should be, of steady augmentation), who published, as her first paper, three new species of Californian plants in the earlier part of the Bulletin (among them a second *Acanthomintha*, confirming the genus, with a difference), contributes another to No. 3, chiefly from her own discoveries. The most interesting one is her *Nemacladus rigidus*. She has also

done excellent service—bibliographical and critical—by looking up all the extant materials of the various genera and species published, during a series of years, by the venerable Dr. Albert Kellogg in the several volumes of the Proceedings of the Californian Academy, as well as in some out-of-the-way and quite unscientific and ephemeral journals or newspapers,—comparing such specimens as could be found with the neat drawings which Dr. Kellogg delighted to make. These drawings are very much better than the rude reproductions of them which were given in the Proceedings, are more numerous, and are generally more helpful than the descriptions in the work of determination. The labor of looking up these scattered publications and of digesting the bibliography must have been very considerable. But Mrs. Curran, with what help she could obtain, has brought them all together in 23 pages of the Bulletin, with the needful references, appending the synonymous name, where there is any known to her. Botanists who have to do with this troublesome matter must heartily thank Mrs. Curran for this conscientious piece of work. Without this exemplification some botanists might have found it difficult to believe that Dr. Kellogg's *Linum trisepalum* is *Helianthemum scoparium*, his *Ludwigia scabriuscula* the *Ammannia latifolia*, his *Gnaphalium Nevadense* the *Antennaria dioica*, his *Egletes Californicus* the common *Bahia lanata*, alias *Eriophyllum cæspitosum*, and his *Heterocodon minimum* the *Alchemilla arvensis*, not "*Specularia biflora*." Also, that the new genera *Melarhiza*, *Partheniopsis*, *Tesserantherum*, and *Ranapalus*, are founded respectively upon the *Wyethia helenioides*, *Venegazia*, *Frasera speciosa*, and *Herpestis rotundifolia*. It is helpful, also, to have in the Bulletin copies of the dozen plates, the greater part colored and of new Lower Californian plants, long ago prepared for the Hesperean, but we believe not published. If they did appear, along with the descriptions, in this "monthly magazine published in San Francisco in earlier years," it is unlikely that the botanical world knew or could have known anything of them. And the same must be said of "the columns of the San Francisco Rural Press,"—hardly a scientific vehicle.

In thus noticing, as it comes in our way, the botanical work of a scientific pioneer on the Pacific coast, we should not withhold our tribute of respect and admiration for this zealous, wholly disinterested, and simple-hearted lover of nature, who merely wished to do what he could for the advancement of our knowledge of the Californian flora, under conditions—such as the want of books and collections—which would not improperly have kept back almost any other equally ardent naturalist.

The ample remainder of the third number and the whole of the recent issue of the fourth consists of "Studies in the Botany of California and parts adjacent," by Edward Lee Greene. They show a quickness quite equal to the author's well known quickness and acuteness in observation. Besides the interesting new material here elaborated—much of it gathered in an enterprising expe-

dition by boat to the islands of Lower California—there is a good deal of reconstruction of old species, a large number of new ones, and several new or restored genera of plants. Valuable as these contributions to our botany must be, we suppose that more time for elaboration, less confidence as to specific distinctions, and a more restrained judgment about genera might have made them better. Yet opinions will naturally differ in botany as well as upon other subjects. The present writer, for one, would not willingly found a genus upon an outlying plant which appears to differ from *Draba* only in its late-dehiscent or possibly indehiscent silicle, and another upon a wingless *Thysanocarpus* (which even Nuttall with all the loose ideas of his later years about genera had no thought of separating); still less would he have thought of a probable junction of these two proposed genera into one upon a "half-anticipation" of an unseen second ovule in the *Thysanocarpus*. Nor would he accede to the restoration of Nuttall's genus *Eucrypta*, nor readily believe that the genus *Eschscholtzia* comprises as many as ten definable species. As to *Mimulus*, although Mr. Greene's discovery that the capsule of *Diplacus* dehisces first and mainly by the upper suture certainly strengthens the claim of the latter to generic rank, there are no new reasons for reinstating *Eunanus*, nor for setting up *M. pilosus* (or *M. exilis*) as a genus. On going over the whole ground anew, with all the extant material, and with all the impartiality the present writer can muster, he still is of the opinion that *Mimulus* is best treated as a multiform genus.

On the other hand there cannot be a better genus than *Bebbia*, Greene (and our associate Mr. Bebb has well earned the honor); and it is not Mr. Greene's fault nor that of Dr. Cooper (who both long ago stated that *Carphephorus junceus*, Benth., had yellow flowers) that the genus had not already taken its place. If the writer was slow of belief, with only the dried specimens before him, he was at once convinced when he came upon this striking plant, full of golden bloom, in the Grand Cañon of the Colorado. Our idea of the affinity of this genus, however, is quite unlike that of Mr. Greene, and will in due time be recorded. Mr. Greene refers to a "sunflower-like odor," apparently of the herbage; but he makes no mention of what was to us a most attractive characteristic, namely, the delicious aroma, like that of *Acacia Farnesiana*, which its blossoms exhale. A. G.

3. *A Systematic Catalogue of the Flowering Plants and Ferns indigenous to or growing wild in Ceylon:* Compiled by HENRY TRIMEN, M.B., F.L.S., Director of the Royal Botanic Gardens, Ceylon. Colombo, 1885. pp. 137, 8vo. Separately issued from the Journal of the Ceylon branch of the Royal Asiatic Society.—Mr. Trimen, the successor of the late Mr. Thwaites at the noted and charmingly situated establishment at Pérádeniya, has set himself actively to the work of mastering the botany of Ceylon, and has now brought out this catalogue of 1071 genera, phænogamous and vascular cryptogamous, besides two of *Characeæ*, and

of their known Ceylonese species, with some synonyms. The naturalized species are enclosed in brackets; the endemic species, by a happy thought, are in small capitals, which at once catch the eye. A. G.

4. Hon. George W. Clinton, probably the oldest native botanist of this country, a man of remarkable personality and attractiveness, died at Albany—the city of his birth and boyhood—September 7, at the age of about 78. Some biographical notice of Judge Clinton, and of the late Charles Wright, may be expected in the January number of this Journal. A. G.

IV. Astronomy.

1. *Identity of Denning's and Biela's comets.*—In the *Observatory* Mr. Denning and Captain Tupman discuss the question whether these two comets may not be the same body, and are led to believe their identity probable. The two comets have their line of nodes nearly coincident, the ascending node of the one being very nearly the descending node of the other. The earth is supposed to have thrown the comet from one orbit into the other, the radius vectors being made to equal that of the earth by minor disturbances of Jupiter.

If however the radiants for the two comets be compared they will be found to differ in position by a distance of 125° or 130°, and this distance is a measure of the necessary disturbing power of the earth in order to throw the comet from one orbit into the other. Either comet, it will be found by a simple computation, would have to come much nearer to the earth's center than 4000 miles in order to suffer such a perturbation of orbit.

V. Miscellaneous Scientific Intelligence.

1. *Meeting of the American Association for the Advancement of Science at Ann Arbor, Michigan.**—The thirty-fourth meeting of the American Association for the Advancement of Science convened in Ann Arbor on Wednesday, August 26, and adjourned on Tuesday evening, September 1. The sessions were held in the halls and apartments of the University, which were found unusually commodious, being spacious, accessible and quiet, and the large University hall seating 3,000 persons. The total number of members registered during the meeting was 364, and the total number of new members elected was 153. A very large number of visitors attended the meeting in addition to the above lists. The total number of papers entered for the meeting was 179; and of these 176 were read, including one illustrated evening lecture. Of these papers there were read before the section of Mathematics and Astronomy, 12; before that of Physics, 23; that of Chemistry, 18; of Mechanical Science, 12; of Geology and Geography, 27; of Biology (which was made to include all paleontological

* The editors are indebted for this sketch to Professor A. Winchell, of Ann Arbor.

papers), 32; of Histology and Microscopy, 4; of Anthropology, 26; of Economic Science and Statistics, 21. The lecture was on Friday evening by Capt. E. L. Corthell, on "The Interoceanic Problem and its Scientific Solution," with lantern illustrations. The address of retiring President Lesley was delivered on Wednesday evening to a large audience in University Hall, the vice-presidential addresses having been mostly given during the afternoon.

The arrangements effected by the local committee appeared to have been perfect, and were the subject of universal commendation. All the usual preparations of rooms, blackboards, tables, sign-boards and the like were provided, and there was opened, next door to the Permanent Secretary's office, a post-office, express office, telegraph office and telephone office. On the same floor was the office of the committees on excursions and on transportation. The insufficiency of the hotel accommodations led to the opening of many private houses—not a few for both lodgings and board—and a common restaurant with a capacity of 300 was organized for such as desired it. Many citizens were also free entertainers. On Thursday evening a public reception was given at the Court House—a new fine building with imposing staircases—whose two stories and basement had been decorated with elaborateness and artistic taste, which won praise from the thousand guests in attendance. On Friday afternoon the city gave also an elegant lawn party on the campus. On Saturday 400 guests, invited by the liberality of Detroit, enjoyed, free of all expense, an excursion to Detroit and thence by the steamer Northwest up the Detroit River and Lake St. Clair, viewing the government improvements in the lake and the unique line of summer houses on the islands; and thence to Marine City. At this point are very extensive salt works, based on an enormous supply of rock-salt of the Salina Group, found here 1633 feet below the surface, and having a known thickness of over 115 feet. After an inspection of these works, for which every facility was provided by the proprietor, Mr. C. McElroy, the excursion proceeded to St. Clair, where an elegant dinner was served at the "Oakland House," a vast summer hotel, whose business is based on a mineral-well supplied from the Huron Group and rich in sulphur. Returning from here, a lunch was served on board, speeches of greeting were made and replied to, and a delightful day was ended with the return at 9 o'clock. A general excursion was also arranged for September 2d to Mackinac and thence in various directions. Entertainments of less general character were numerous. A reception was given to the geologists and many of their friends on Friday evening by Professor A. Winchell, and one to the chemists and their friends on Monday evening by Dr. Prescott. The botanists were taken by Professor Spaulding on an excursion to the Tamarack Swamp. Invitations to lunch and dinner were abundant; and carriage rides about the city and suburbs were quite general. The principal interest in these details is the demonstration that the Association can obtain ample conveniences

and enjoyments in a city with a population of less than ten thousand. Indeed, Ann Arbor could have accommodated a thousand guests as easily as five hundred.

It was generally remarked that the scientific work of the meeting was good. An unusually large proportion of Fellows was present. Fewer papers than usual had to be rejected; and there were several papers of great and permanent importance, as will appear from the particular reports. The section of Histology and Microscopy was merged in that of Biology by request of the section itself. Resolutions were adopted expressing a high appreciation of the value of the work of the "Coast and Geodetic Survey," and a hope that criticisms of the work might be left by the government to competent scientific experts.

The subjects of the addresses before the several sections by the vice-presidents were as follows: Professor W. R. NICHOLS, to the Chemical section, on Chemistry in the service of public health; Professor EDWARD ORTON, to the Geological section, on unfinished problems relating to the geology of coal; Professor J. BURKITT WEBB, of Ithaca, to the Mechanical section, on the second law of Thermo-dynamics; Dr. B. G. WILDER, of Ithaca, to the Biological section, on an educational museum of Vertebrates; Professor S. H. GAGE, of Ithaca, to the section of Microscopy and Histology, on the limitations and value of histological investigations; Mr. W. H. DALL, of Washington, to the Anthropological section, on the native tribes of Alaska; Mr. EDWARD ATKINSON, of Boston, to the section of Economical Science, on the application of Science to the production and consumption of food. [Abstracts of these addresses, together with notes on many papers read at the meeting, are given in the number of Science for September 11 (No. 136) and the address entire of the retiring president, Professor Lesley, in the number for August 28.]

Buffalo, New York, was selected for the meeting of the Association in 1886—where the meetings of 1866 and 1876 were held—and Wednesday the 18th of August appointed for the opening session.

Professor EDWARD S. MORSE, of Salem, Mass., was elected President, and the following for Vice-Presidents of the different sections: Professor J. W. GIBBS, of New Haven, Mathematics and Astronomy; Professor C. F. BRACKETT, of Princeton, Physics; H. W. WILEY, of Washington, Chemistry; O. CHANUTE, of Kansas City, Mechanical Science; Professor T. C. CHAMBERLIN, of Washington, Geology and Geography; Professor H. P. BOWDITCH, of Boston, Biology; HORATIO HALE, of Clinton, Ontario, Anthropology; JOSEPH CUMMINGS, of Evanston, Ill., Economic Science and Statistics.

List of Papers accepted for Reading.

1. *Astronomy. Mathematics, Physics.*

H. A. NEWTON: Effect of small bodies passing near a planet upon the planet's velocity.

D. P. TODD: On a rare sun-spot, observed 1885, May 19, 21 and 22; The audible circle,—a new device whereby the settings of an astronomical instrument may be made by the ear.

W. HARKNESS: On the flexure of transit instruments.
G. W. HOUGH: Description of a printing chronograph.
J. BURKITT WEBB: Polar versus other coördinates.
J. HAYWOOD: The visible shadow of the earth.
S. P. LANGLEY: Spectra of some sources of invisible radiation, and on the recognition of hitherto unmeasured wave-lengths.
J. A. BRASHEAR: A practical method for working rock-salt surfaces for optical purposes.
J. W. MOORE: The direct optical projection of electro-dynamic "lines of force;" The optical projection of electro-dynamic phenomena.
H. S. CARHART: On surface transmission of electrical discharges.
E. L. NICHOLS: A spectro-photometric analysis of the color of the sky; Chemical behavior of iron in the magnetic field.
H. E. ALVORD: Telemetric aid to meteorological records.
W. FERREL: Psychrometry.
C. H. CHANDLER: A new harmonograph.
T. F. JEWELL: Apparent resistance of a body of air to a change of form under sudden compression.
H. W. EATON: The relation of vanishing and permanent magnetism.
T. C. MENDENHALL: Note on electrometers and atmospheric electricity.
A. E. DOLBEAR: On the contact theory of electricity; On an incandescent electric lamp for projection; On a new galvanic element.
A. J. ROGERS: Electrolysis of salt of the alkalies and alkaline earths.
C. K. WEAD: Exhibition of a combined spectro-photometer and ophthalmospectroscope.
C. J. REED: Exhibition of an apparatus for demonstrating the laws of falling bodies.
C. H. ROCKWELL: Some practical results in determining time and latitude with the almucantar.
H. H. CLAYTON: Weather changes of long period.
T. BASSNETT: Parallax of the sun.
S. S. HAIGHT: Rapidity of calculation.

2. *Chemistry.*

T. TAYLOR: On the crystals of butter and other fats.
A. B. PRESCOTT: Control analyses, and limits of recovery in chemical separations.
E. D. CAMPBELL: A colorimetric method for the estimation of phosphorus in iron and steel.
W. A. NOYES: On para-nitro-benzoic-sulphinide.
W. H. WILEY: Estimation of acetic acid occurring with lactic acid in sour milk or *kumys;* Composition of *kumys* made from cow's milk; Honey and its adulterations.
E. H. COWLES, A. H. COWLES and C. F. MABERY: On the electrical furnace and the reduction of the oxides of boron, silicon, aluminium and other metals by carbon.
W. A. WITHERS: Chemical conversion scales.
H. B. BATTLE and F. B. DANCY: Consideration of the atomic weights of some of the elements found in agricultural analysis, and their application to conversion tables to facilitate calculation.
Note on the estimation of water in glucoses, honeys, etc.
HELEN C. DES. ABBOTT: A chemical study of Yucca angustifolia.
A. V. E. YOUNG: Thermo-chemical analysis of the reaction between potassic hydrate and common alum.
F. P. DUNNINGTON: A sponge-like mass containing titanic acid; A simple method of fixing crayon drawings on paper.
J. W. LANGLEY: On the concentration of certain acid radicles by the differential action of chemism.
O. C. JOHNSON: Negative bonds and rule for balancing equations.
J. W. PIKE: Chemical and thermo-chemical relations of the gases of the atmosphere in the disintegration and metamorphism of rocks.
C. J. REED: Graphical representation of the relation between valence and Mendelejeff's periodic law.

C. G. WHEELER: Action of carbonic anhydride in aqueous solution on some minerals, and modifying influence of sodium chloride.

3. *Mechanical Science.*

L. S. RANDOLPH: Strength of stay-bolts in boilers.
D. P. TODD: On a universal form of pressure motor.
S. S. HAIGHT: Use and value of accurate standards for surveyors' chains.
J. BURKITT WEBB: The lathe as an instrument of precision; The economy of accurate standards.
C. J. H. WOODBURY: Experiments upon the coefficient of efflux of automatic sprinklers.
F. C. WAGNER: Electric light tests.
M. E. COOLEY: Testing indicator springs; A new smoke-burning device.
R. H. THURSTON: Cylinder condensation in steam engines; A Prony brake for governing powerful steam engines.
A. HOGG: Deep water at Galveston, Texas, and how to secure it.

4. *Geology, Paleontology, Mineralogy.*

A. WINCHELL: Geology of Ann Arbor; *Cœnostroma* and *Idiostroma*, and the comprehensive character of Stromatoporoids; Sources of trend and *crustal surplusage* in mountain structure.
W. B. TAYLOR: A probable cause of the shrinkage of the earth's crust.
S. G. WILLIAMS: Westward extension of rocks of the Lower Helderberg period in New York.
J. D. DANA: Lower Silurian fossils in a limestone of the original Taconic.
L. E. HICKS: Structure and relations of the Dakota Group.
F. D. CHESTER: Results from a study of the gabbros and associated amphibolites in Delaware.
A. D. CRANDALL: Occurrence of trap rock in eastern Kentucky.
A. H. WORTHEN: The Quaternary deposits of Illinois.
G. K. GILBERT: Post-glacial changes of level in the basin of Lake Ontario.
H. S. WILLIAMS: Classification of the Upper Devonian.
E. ORTON: The gas and oil wells of northwestern Ohio; Record of the deep well of the Cleveland Rolling Mill Co., Cleveland, O.
W. MCADAMS: The Lœss and drift-clays.
W. B. DWIGHT: Discovery of fossiliferous Potsdam limestone at Poughkeepsie, N. Y.
E. W. CLAYPOLE: The materials of the Appalachians.
T. STERRY HUNT: Apatite deposits in Laurentian rocks.
J. C. BRANNER: Glaciation of the Lackawanna Valley.
C. WACHSMUTH: The presence or absence of underbasals can be ascertained from the columns.
E. D. COPE: On the brain and auditory organs of a Permian Theromorph Saurian.
A. S. TIFFANY: The Corniferous or Upper Helderberg Group of Scott County, Iowa and Rock Island, Ill., with a list of fossils; The Chemung Group at Burlington, Iowa, with a list of its fossils.
N. H. WINCHELL: Notice of Lingula and Paradoxides from the red quartzites of Minnesota.
G. F. KUNZ: A new mass of meteoric iron from Charleston, Kanawha County, West Virginia; Mineralogical notes: Tourmaline locality at Rumford, Maine; Pseudomorph of feldspar after leucite (?) from Magnet Cove; On a remarkable rough collection of diamonds; Native antimony and its associations at Prince William, New Brunswick.

5. *Botany, Zoology, Microscopy.*

E. STURTEVANT: Observation on the hybridization and cross-fertilization of plants; Germination studies.
C. E. BESSEY: Further observations on the adventitious inflorescence of *Cuscuta glomerata;* Question of bisexuality in the pond-scums (*Zignemaceæ*).
C. R. BARNES: Process in fertilization in *Campanula Americana.*

H. G. BERGER: Biological deductions from a comparative study of the influence of Cocaine and Atropine on the organs of circulation; Structure of *Glattidea pyramidata* (Stien) Dall.

C. V. RILEY: The song-notes of the periodical Cicada (*Cicada septendecim* L.), and the mechanism by which they are produced; Some popular fallacies and some new facts regarding *Cicada septendecim;* On the parasites of the Hessian-fly (*Cecidomyia destructor* Say).

A. W. BUTLER: The periodical Cicada in southeastern Indiana; Observations on the musk-rat.

J. C. ARTHUR: Proof that Bacteria are the direct cause of the disease in trees known as pear-blight.

S. H. GAGE and S. S. PHELPS GAGE: Aquatic respiration in soft-shelled turtles (*Aspidonectes* and *Amyda*).

E. D. COPE: Phylogeny of the placental Mammals.

T. J. BURRILL: The mechanical injury to trees by cold.

J. M. COULTER: On the appearance of the relation of ovary and perianth in the development of dicotyledons.

C. S. MINOT: A new membrane of the human skin; Organization and death; Morphology of the supra-renal capsules; The structure of the human placenta; Evolution of the lungs.

D. H. CAMPBELL: The development of the prothallium of ferns.

J. B. STEERE: The importance of individual facts of environment in the formation of groups of animals.

B. G. WILDER: Experiments antagonizing the view that the serrulæ (serrated appendages) of *Amia* are accessory organs.

H. AYERS: On the structure and functions of Sphæridia of the *Echinidii;* On the carapax and sternum of the Decapod Crustacea.

W. G. FARLOW: Notes on some injurious Fungi of California.

D. E. SALMON and T. SMITH: A new Chromogenous Bacillus (*Bacillus luteus suis*).

J. B. WEBB: Entropy; The life of the universe.

W. H. WALMSLEY: Photo-micrographs on gelatine plates for lantern projection.

T. J. BURRILL: Photo-micography work with high powers.

C. P. HART: A new, cheap and quickly constructed adjustable microtome.

R. HITCHCOCK: Optical arrangements for photo-micrography, and remarks on magnification.

6. *Anthropology.*

J. O. DORSEY: A visit to the Siletz agency; Primary classifiers in Dhegiha and cognate languages; Indian personal names.

A. W. BUTLER: The remains of San Juan Teotihuacan.

N. H. WINCHELL: A supposed natural alloy of copper and silver from the north shore of Lake Superior, in Minnesota.

W. C. WYMAN: Exhibition of copper implements.

ALICE C. FLETCHER: Sacred war tent and some war customs of the Omahas: An average day in camp.

F. LA FLESCHE: Laws and terms of relationship of the Omahas.

W. MCADAMS: Exploration of recent Indian mounds in Dakota; Ancient pictographs in Illinois and Missouri; Explorations in the great Cahokia mounds.

H. GILMAN: Burial customs of our aborigines.

W. L. COFFINBERRY: Exhibition of specimen with notes.

J. W. SANBORN: Customs, language and legends of the Senecas.

W. M. BEAUCHAMP: Permanence of Iroquois clans and sachemships.

Mrs. E. A. SMITH: Significance of Flora to the Iroquois; Who made belt wampum?

M. L. ROUSE: Music in speech.

G. H. PERKINS: The stone axe in the Champlain Valley; Stone implements from Vermont.

C. S. MINOT: The number habit; Are contemporary phantasms of the dead to be explained partly as folk lore?

F. W. PUTNAM: Ornaments made of pieces of human skulls, from a mound in Ohio; Proper methods of exploring mounds.

W. ZIMMERMAN: The degeneracy of races.

W. DEHASS: The animal mounds of Wisconsin.

7. *Economic Science and Statistics.*

H. E. ALVORD: Relative value of human foods.

C. V. RILEY: A new method of counteracting the ravages of locusts or so-called "grasshoppers" (Acrididæ); The present status and future prospect of silk-culture in the United States.

C. REEMELIN: City Government.

E. ATKINSON: Insurance against loss by fire; Competition and coöperation synonymous terms.

E. B. ELLIOTT: The silver question; Electric lighting.

J. W. HOYT: On the need of a systematic reorganization of the executive departments of the government in the interest of science and of public economy.

C. W. SMILEY: Some defects of our Savings Bank system and the need of Postal Savings Banks in the United States.

MRS. ELLEN H. RICHARDS: An illustration of a method of teaching elementary science in grammar schools.

2. *U. S. Coast and Geodetic Survey.*—The resolutions with regard to the United States Coast Survey, passed at the recent meeting of the American Association at Ann Arbor, without a dissenting voice, are in accord, we believe, with the views of scientists throughout the country. The survey has had great influence in promoting the progress of high science in the land through its demand for the best abilities in the departments of mathematics, physics, hydrography and geodesy, in order to carry forward its work, and through the investigations it has been compelled to undertake for the improvement of its methods, the elaboration of its observations and the perfecting of its results. No department of work under the government requires greater exactness and a wider and profounder range of knowledge. For thirty years and more, commencing under Professor Bache, its annual reports have contained, not only charts of hydrographic work in great numbers, but also papers of high scientific merit bearing on the various questions arising out of the investigations in progress, and others fundamental to those investigations; and this has continued to be true to the latest issue under Mr. Hilgard. The work of the Coast Survey, as outlined by Bache and carried forward by his successors, has a unity and a completeness which should be preserved in its future scope and management. Besides elevating the science of the country, it has tended also to exalt in foreign lands the standing of American science. Any crippling of the Survey in the present unfinished stage of its work would therefore be a national calamity.

The demand, in the vote of the Association, that the head of the Coast Survey (and by inference the superintendents of other scientific work) should have the highest possible standing among scientific men, and should command their entire confidence, is in accordance with the "civil service" principles of the country. Sure destruction to the usefulness and reputation of the scientific departments—for example, those of the Coast Survey, the Smithsonian Institution and the Geological Survey—would follow their subjection to the control of persons without thorough scientific education, for it would be quite sure to end in subjection to the debasing influences of political ambition.